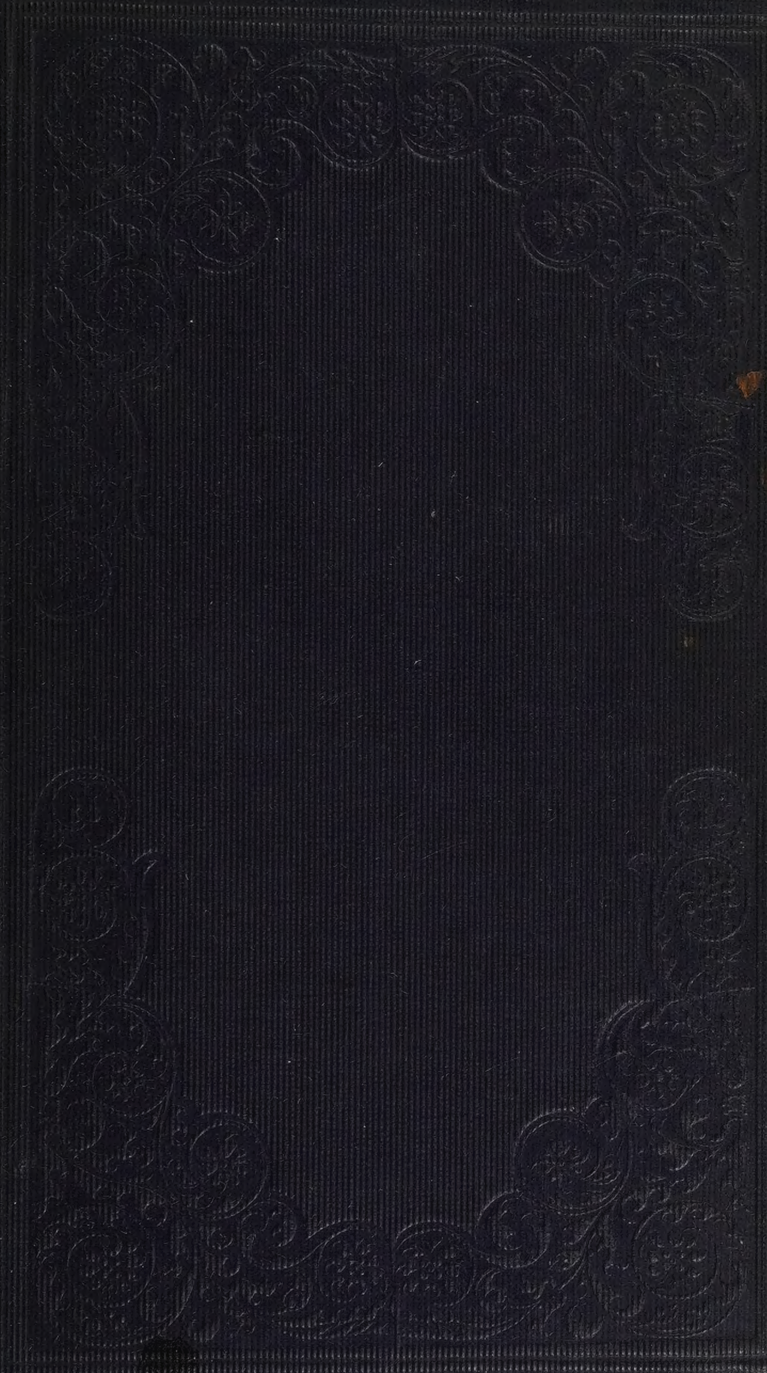

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BRAITHWAITE'S RETROSPECT.

VOL. XLVII. JANUARY—JUNE, 1863.

THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

EDITED BY

W. BRAITHWAITE, M.D.,

LATE LECTURER ON MIDWIFERY AND THE DISEASES OF WOMEN AND CHILDREN AT THE LEEDS SCHOOL OF MEDICINE, ETC.

AND

JAMES BRAITHWAITE, M.D. LOND.

VOL. XLVII. JANUARY—JUNE,
1863.

LONDON:

SIMPKIN, MARSHALL, AND CO.

EDINBURGH: OLIVER AND BOYD. DUBLIN: HODGES AND SMITH.

MDCCCLXIII.

Letters for the Editors, to be addressed
Dr. Wm. Braithwaite, Clarendon House, Clarendon Road, Leeds; *or*,
Dr. James Braithwaite, 1, Woodhouse Square, Leeds.

Parcels of Books, &c., to
Messrs. Simpkin, Marshall, & Co., London.

D. I. ROEBUCK, PRINTER, 14, TRINITY STREET, LEEDS.

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AFFECTIONS OF THE SYSTEM GENERALLY.

ACUTE RHEUMATISM.—Our aim in treating a case of acute rheumatism should be to induce alkalinity of the urine as speedily as possible. As soon as a full alkaline action has been induced, as indicated by alkalescence of the urine, the heart is safe, and no fear need be entertained of its suffering from the irritation of the rheumatic poison. In a case of acute rheumatism in which febrile action runs high, and the urine is loaded with lithates, it is not an easy matter to induce alkalinity of the urine. To be of any service alkalies must be given in large and repeated doses. Ordinarily, two drachms of some alkaline carbonate or its salts, if repeated every three or four hours, will suffice to induce alkalinity of the urine within twenty-four hours. Where once alkalinity of the urine has been induced, the dose of the alkali should be greatly diminished. When the urine is alkaline, and has remained so two or three days, and when the skin and pulse no longer indicate a great degree of fever, even though the tongue remain furred, the administration of cinchona will prove serviceable. The virtue of alkalies in acute rheumatism depends upon the power, 1st, of acting as restorers of the alkaline condition of the system; 2nd, of assisting to maintain the solubility of the fibrine, and thereby preventing its deposition on the valvular apparatus of the heart; 3rd, their action as sedatives, of calming the action of the heart and arteries. Practically it is found that the joint action of soda and potash is much more satisfactory in its results than the action of either of them taken separately. As ammonia exercises a power of maintaining the fibrin of the blood in solution, beyond comparison greater than that exerted by potash and soda, never fail to give it in conjunction with the other remedies. (Dr. H. W. Fuller, p. 24.)

Nitrate of Potash.—The fact of the increase of fibrine in the blood in acute rheumatism, from two parts in a thousand to six or more, is well known, but it is not so generally known that the saline constituents, normally about twelve parts in a thousand, descend often as low as eight or even seven. A man was bled from a vein, who had swallowed an ounce of nitre in solution, and the blood came from the vein almost as bright as arterial, and continued fluid. Applying these facts to the treatment of acute rheumatism by nitrate of potash, we see how beneficial the results are likely to be. Practice quite bears out what *á priori* might be expected. Other salines act extremely well, but none in my opinion so well as does nitrate of potash. Brisk purgatives in some cases are desirable at the commencement of treatment, and in some cases opium is of value; but except these, no other medicinal treatment is requisite than the administration of nitre in moderate but repeated doses. (Dr. W. R. Basham, p. 18.)

ASCITES AND ANASARCA.—*Early employment of Paracentesis.*—Instead of looking upon the removal of the fluid by tapping, or by acupuncture, as the last plan we should resort to, we ought, in cases of ascites and anasarca, to remove it early by one of these means. It is not a palliative, but an important curative agent. Until the fluid is removed, the secretory structure of the kidney is incapable of acting from congestion, and diuretics are useless. The steam bath, however, and hydrogogues, would naturally be tried in the first instance, but if they do not speedily produce the desired effect the means recommended should be at once resorted to. (Dr. J. B. Nevins, p. 39.)

GOUT.—Some cases of gout arise from a *rapid* accumulation of lithic acid in the blood; the general health is good, the patient florid, and there are symptoms of high fever, and acute local inflammations. The second class of cases (atonic or chronic gout) are slow in their origin and progress; the general health is indifferent, the patients are of pallid complexion and lax fibre, there is an absence of severe febrile disturbance, and the local inflammatory action is less acute. The first or acute case may be called gout arising from excessive formation of uric acid, and the second or chronic case gout from defective excretion of uric acid. In the cases of *acute gout* in otherwise healthy persons—cases engendered by the excessive formation of uric acid in the system, as a consequence of an over abundant and over stimulating diet, deficient excercise, and an undue amount of sleep—little more is needed than to alter the habits of life, and to administer such medicines as may be requisite to induce a full action of the excretory organs. The cause of the accumulation of uric acid is thus arrested. In a remedial point of view, the most important thing is to produce full action of the excretory organs; for this purpose a hot-air bath and such a pill as the following may be given every night: two grains of the acetic extract of colchicum, three grains of calomel, and one grain of the watery extract of aloes. In the case of *chronic gout* the whole history points to defective excretion. There is no history of over-loading the system, and no evidence of an excessively rapid formation of lithic acid in the blood. From the state of constitution in these cases large quantities of colchicum and alkalies are inadmissible, and hot-air baths would produce exhaustion. Warm baths containing soda are, however, useful Alkalies may be given in small quantities. The diet should be fish and beef-tea, if there are many sub-acute gouty symptoms present. The following is a good prescription:—Carbonate of magnesia and sulphate of magnesia, of each one scruple; solution of taraxacum, one drachm; colchicum wine, twelve minims; compound infusion of gentian, ten drachms: to be taken every six hours. A gouty man needs all the nervous power he can command to enable his various organs to do their work efficiently, and if he sits down to dinner in a state of exhaustion, his functions cannot but be imperfectly performed. A weakly man cannot be too cautious not to overtax his strength, and so to impair his power of digestion. If, by force of circumstances, he is compelled to sit down to dinner unduly exhausted, he should commence with a glass of sherry or some slight stimulant, and should be more than ordinarily abstemious. If the patient habitually passes his time in impure town air, as is generally the case, and takes deficient exercise, a change of habit in these particulars must be imperatively insisted on. (Dr. Fuller, *Lancet*, *Dec.* 13, 1863, *p.* 641.)

HYDROPHOBIA.—*Electricity.*—A patient suffering from hydrophobia, and so far advanced in the disease as to have become entirely unmanageable, was secured and bound to a mattress, and a copper wire being wound round both feet, the conductor of the negative pole was attached to this wire, and the conductor of the positive pole, through a sponge saturated with vinegar and salt, was applied to the throat, and over the spine and body generally, with the full power of the battery. This produced instantaneous cessation of the spasms, and while under the influence of electricity the patient willingly drank liquids, and was free from the usual horror of them. The current was continued for some hours, with intervals of omission; the patient began to collapse, then vomiting and purging freely followed, and finally he fell asleep, and recovery ensued. This case, which occurred in New York under the care of Dr. Lassing, seems extraordinary, and we should be glad to hear of a repetition of the experiment in England. (Dr. H. Lassing, p. 53.)

Scarlet Fever.—*Chlorine Mixture.*—The following formula for the chlorine mixture, so useful in scarlet fever cases, is worth noting:—℞. Potassæ chloratis ʒij; acidi hydrochlorici ℥ij; aquæ destillatæ ℥vj. Ut fiant ℥viij, misce et solve. Dose (for an adult), ♏xv. The extempore prescription for the mixture would then be:—℞. Liquor chlorinii ʒij; aquæ ad ℥viij. Misce. Dose for an adult, ℥j every three hours; for children, from ʒj to ℥ss. (Mr. W. W. Morris, *British Med. Journal, Dec.* 6, 1862, *p.* 600.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Delirium Tremens.—Is a specific form of nervine poisoning, of which alcohol is at once the predisposing and exciting cause. The blood is poisoned by unchanged alcohol, and is rendered perfectly unfitted for the proper nutrition of the brain by long want of proper food, and by the retention of numerous matters which, in a state of health, are at once excreted, either unchanged or after undergoing some metamorphosis. Hence the proper indications for treatment are, not to trust entirely to nature, or to treat some accompanying disorder, but at once to cut off any further supply of the poisonous substance by which the blood is infected; to strive by all means to get nourishing food administered; and to take proper measures for the thorough depuration of the blood, and the elimination from the system of the many effete matters with which it is crowded. The fact of the natural subsidence of the paroxysm of delirium, quite independently of art, explains the apparent success of such different modes of treatment as have been recommended. The free exhibition of some alcoholic liquor is probably the most frequently adopted practice in this country. This treatment, though recommended by several eminent men, is not warranted by the experience of those who have had unusually large spheres of observation. Probably the profuse perspirations of delirium tremens are one of the natural means by which the depuration of the blood is effected. Hence, if the temperature becomes decidedly low, and more especially if along with this there is a decided diminution or arrest of the cutaneous exhalation, give stimulating diaphoretics, such as a mixture of camphor and ammonia. (Dr. W. Perrie, jun., p. 55.)

Epilepsy.—*Belladonna.*—At first the dose should be very small, and gradually augmented until the pupil shows signs of its action, and the patient complains of both alteration in sight and dryness of throat; the dose must then be diminished until these effects are so far lessened as to cease causing absolute discomfort. The other toxic effects of belladonna are wholly uncalled for. Immediate and palpable beneficial results must not be looked for. At first the number of fits may even increase, but after the drug has been taken six or eight weeks, if any amelioration occur, it will be decided and progressive. At first the drug should be given in an eighth of a grain dose, three times, or only twice daily, for a week; then a quarter of a grain for fourteen days; a third for the next fourteen days, at which time its physiological action will in most cases be manifest. It is well to halt at this dose, for two or three months, slightly increasing the dose if the patient shows diminished susceptibility to its influence. To those who wish to use a preparation of uniform strength, the salts of atropia are easily procurable. The hundred-and-twentieth of a grain of valerianate of atropia may be given as a commencing dose. Trousseau gives a centigramme of the extract, and an equal quantity of the powder of belladonna for the first month, in the evening of each day; during the second month he gives two such pills at the same time; and during the third month, three pills. If, at the end of six or nine months, the frequency of the fits is decreased, he increases the dose. He asserts that, of 120 patients, he has cured twenty. (Dr. J. S. Ramskill, p. 43.)

Noisy and Restless Insanity.—*Hydrocyanic Acid.*—A case is related in which the patient was noisy and restless, incessantly in motion, constantly shouting and screaming, and for long periods sleepless. She was completely delirious, and had not during the day a moment's repose. Four minims of dilute hydrocyanic acid were given every hour, in half-an-ounce of camphor mixture. Immediately after the first dose she became quiet and still, and slept at night. She continued better until the medicine was discontinued, when she at once relapsed to her old state. This may prove a valuable hint in some of these distressing and unmanageable cases. (Dr. K. Mc'Leod, p. 65.)

Traumatic Tetanus.—*Nicotine.*—The employment of nicotine in cases of traumatic tetanus, as recommended by the Rev. Professor Haughton, has been further tried. For a considerable period after commencing the nicotine treatment the disease progressed in intensity, but still a decided impression and relaxation of spasm followed upon each separate employment of the remedy. Recovery ultimately ensued. The observations of Mr. Haughton, given in our last volume, are completely endorsed by the author, under whose care the cases occurred. (Mr. J. Tufnell, p. 46.)

AFFECTIONS OF THE CIRCULATORY SYSTEM.

Thoracic Aneurism.—*Iodide of Potassium.*—An interesting case of thoracic aneurism making its way forward through the parietes of the chest is related, in which the following treatment was adopted with great benefit. The patient was ordered to take five grains of iodide of potassium three times a day; to keep his bed, and to restrain as much as possible the quantity of liquids taken in the way of drink. Three days later, the iodide was increased to seven grains and a half three times a day. In six days, the patient reported himself a great deal better, and in other five days, the dose having meanwhile been increased to ten grains, he was free from pain, difficulty of breathing, and dysphagia, and the soft elevation had almost altogether subsided to the level of the surrounding parts. He was now allowed to get up, the restrictions as to fluids were relaxed, and the dose was increased to fifteen, and subsequently to twenty grains three times a day, the area of dulness meanwhile steadily decreasing. In six weeks the soft elevation had become inappreciable to the eye, though pulsation was still felt over the spot. "The patient was free from pain and difficulty of breathing, and felt himself exceedingly well." The attention of Mr. Roberts was first called to this mode of treating aneurism by Mr. T. Windsor. Some cases have been published showing the advantages of this mode of treatment, by Dr. Chuckerbutty, of Calcutta. These appear to indicate that the beneficial effect of the iodide is owing to its power hitherto wholly unsuspected of increasing the coagulability of the blood. (Dr. W. Roberts, p. 171.)

Enfeebled Heart.—*Digitalis.*—In cases where there is decided feebleness of the heart's action—the heart's sounds being weak, the area of dulness increased, the impulse weak and diffused, and the legs dropsical—digitalis is of the greatest value. Most would use it in such a case with fear, but the drug is a stimulant and tonic, especially when given with iron, and not a depressant to the muscular structure of the heart. The following is a good formula: Tr. ferri muriat., ♏x.; tr. digitalis, ♏x.; spt. æth. chlor., ♏x.; aquæ, ℥j.; ter die. It now seems certain that the action of digitalis is to cause contraction and increased tone of the heart's fibres. It certainly, as observations made by Winogradoff and Traube show, induces arterial contraction; and Kulp's account of the action of digitalis in inflammatory disease, seems to prove that it induces arterial contraction, rendering the hands and face cold. Its action upon the uterus in menorrhagia, and on the arteries in hemorrhages, seems, also, to be of this kind. It is probable that when dangerous symptoms follow the administration of digitalis, it is from its producing a tetanic state of the heart, *i.e.*, stimulating this hollow muscle so strongly to contract, that it does not duly relax, and admit a supply of blood. It is from this action of the drug that it is of use, or said to be, in cases of delirium tremens. It seems to be injurious in sthenic, and beneficial in asthenic cases. (Dr. Handfield Jones, p. 101.)

NÆVUS.—*Tartar Emetic.*—Make a plaster of from sixteen to eighteen grains of tartar emetic, and one drachm of diachylon, and spread a considerable portion of this all over, and somewhat beyond the nævus by means of the back of a strong knife, and keep it *in situ* by means of gummed paper. On the fifth or sixth day, the entire surface of the nævus begins to suppurate, a crust gradually forming, which passes off in about fifteen days, leaving a small cicatrix. (Dr. Zeissl, p. 178.)

Nævus of the Eyelid.—The following is an excellent plan of treating this affection. Having put the child well under the influence of chloroform, heat a needle to dull redness in the flame of a spirit-lamp, and puncture through and through the entire substance of the lid a considerable number of times, the needle being heated as often as necessary during the operation. The needle should be set in a handle, and should have a bulb about one-third of an inch from its point to serve as a reservoir for heat. Moreover, the eyelid should be raised from the globe by means of an ivory spoon, both to protect the eye and to form a firm basis for the lid. (Mr. J. C. Wordsworth, p. 179.)

RHEUMATIC PERICARDITIS.—Ordinarily, when under appropriate treatment friction sounds do not cease, owing to the unorganized state of the effused lymph, the cause is the feeble condition of the patient, and this state of exhaustion of the system is usually the origin of the untoward symptoms by which death in pericarditis is preceded. Hence the necessity of supporting treatment in all cases in which there is feebleness of system. Beef-tea and cinchona seem strange remedies for a case of acute local inflammation, but they are sometimes necessary. "None of the functions of the body, and none of the actions which are necessary to the reparation of the tissues, can go on properly in a person enfeebled beyond a certain point." (Dr. H. W. Fuller, p. 88.)

SEVERE EPISTAXIS.—*Plugging the Nose.*—Pass a piece of dry and compressed sponge, about two inches long, and of sufficient size to pass easily, along the floor of the nares. Secure this in the middle by a piece of tape or twine doubled, when the sponge is fairly pushed by a probe or female catheter through the posterior nares, draw upon the tape, the sponge then takes a direction transverse to the nasal cavity, doubling upon itself and so blocking up, when swelled by the absorption of blood, the posterior nares. The two ends of the tape now passing out of the nostril should be separated, the anterior cavity filled with cotton wool, and the tape then tied across the whole in front of the nose. To remove it push the sponge into the pharynx, and bring it out through the mouth. This plan is only necessary in very severe cases of epistaxis, and is attended with less distress to the patient and inconvenience to the operator, than the usual plan. (Dr. Cahill, p. 181.)

AFFECTIONS OF THE RESPIRATORY SYSTEM.

CHRONIC BRONCHITIS.—The *Verbascum thapsus,* or *Mullein,* a common American plant, has considerable reputation in some parts of that continent in cases of chronic bronchitis, coughs, &c. Dr. Gardner has had prepared a tincture from the flower stalks just as it was coming into seed. This tincture is of a dark rich brown colour, and is found to possess considerable narcotic properties; in fact "more reliable than any narcotic, except opium and salts of morphia." In doses of one drachm to a drachm and a half, it produces calm sleep, and is in no case followed by headache. In coughs and bronchitis, in combination with the usual remedies, it has marked soothing effects, and yet does not suddenly arrest secretion as does opium. It is also useful in some cases of asthma. It is much more active and certain than hyoscyamus, and seems well worthy of trial by the profession.—(*Lancet, Dec.* 6, 1862, *p.* 629.)

THE LARYNGOSCOPE.—Usually the observer has looked through a central hole in the reflector, or he has used only the uncovered eye, but there is much inconvenience in this, as it is very difficult to keep the eye applied to the central aperture when both hands are engaged in the performance of any operation on the throat. All inconvenience is obviated by having the reflector so attached to the frontal band, that it shall come to the middle of the forehead and leave both eyes uncovered. The operator has thus the free and unrestrained use of both eyes, and finds it easy to direct the light into the patient's throat. By throwing the light from the lamp, as it were, into the open mouth of his own image in a looking-glass, and then introducing the faucial mirror, the operator may at once see the reflection of his own larynx, thus performing what may be called auto-laryngoscopy. (Dr. G. Johnson, p. 109.)

Suspended Animation.—From many and conclusive experiments, Dr. Richardson has proved that, in cases of suspended animation, artificial respiration can prove of avail only while the heart is transmitting a pulmonic wave of blood, and that in the absence of this wave, artificial respiration is injurious —injurious to the lungs and fatal to the blood. Artificial respiration alone will not cause rhythmical contraction of the heart; the action of that organ which is seen when the chest wall is removed is owing to the exposure to the air. The best means of inducing a return of circulation is the long-continued application of gentle external warmth. In future researches careful attention must be directed to the elucidation of the question of the real effects of gentle external heat, continued for lengthened periods; for it is found that animals will often recover after several hours' emersion, provided the warmth be continued sufficiently long. If a current of blood can once be made to flow, artificial respiration will do the rest. (Dr. B. W. Richardson, p. 359.)

AFFECTIONS OF THE DIGESTIVE SYSTEM.

Cholera.—*Subcutaneous Injection of Morphia.*—Opium seems of undoubted benefit in cholera, but is so generally rejected immediately from either the stomach or rectum that its full effects cannot easily be obtained. If, however, it is injected beneath the skin, it action will be found to be prompt and efficient, relieving at once the violent abdominal cramps and muscular action of stomach and rectum. ♏xxv. of liq. morph. acet. may be injected with a Wood's syringe, beneath the skin of the abdomen. (Dr. I. Ashe, p. 117.)

Femoral Hernia.—In endeavouring to reduce a hernia I never hesitate to give chloroform, as the immense use which it has proved itself to be, quite overbalances the chance of occurrence of considerable chloroform sickness. In three cases out of four at least the taxis will fail, that is in femoral hernia; it ought not to be prolonged beyond a few minutes, and on no account must force be employed. If you then fail, you must make up your mind to operate, and to do so at once. Opening or not opening the sac is quite a matter of secondary importance compared with the period of strangulation, and the more or less forcible taxis. I invariably open the sac, and have never seen any harm arise from this procedure, and if this is not done in some cases the surgeon cannot be quite sure that a bit of bowel is not left in the sac. (Mr. Prescott Hewett, p. 182.)

Inversion of the Patient.—The taxis and every other means of reducing hernia failing, procure a board of five or six feet in length, and place and fix the patient by straps with his knees flexed over one end of the board. Then invert completely, or nearly so. The tumour will in many cases disappear spontaneously, and in very few will it refuse to return by a little manipulation. The explanation is the traction on the tumour exercised by the weight of the intestines, which of course gravitate from the tumour. (Anonymous, p. 184.)

PROLAPSUS ANI.—*Nitric Acid.*—In those old cases of prolapsus of the rectum in which the sphincter ani is completely relaxed, exercising no constriction whatever on the protrusion, and in which the surrounding skin is relaxed and superabundant, and thrown into numerous large folds, there is perhaps no better plan of treatment than the application of nitric acid. It may be applied to half the protrusion at a time, and the folds of integument removed with scissors. (Mr. C. J. Holthouse, p. 186.)

TAPEWORM.—*Oil of Male-Fern.*—The mode of exhibition of the oil of male-fern is important. Mix well oil of male-fern one drachm; mucilage half an ounce. Let this draught be mixed with one ounce and a half of sweet milk, and taken at bedtime, the patient having omitted the dinner and evening meal of that day. Taken thus on an empty stomach, the mixture is carried speedily into the intestines, to feed, and at the same time poison, the hungry parasite which nestles there. Milk is the favourite food of the worm. If the bowels are constipated, give next morning a dose of castor oil; otherwise not. If necessary this medication may be continued daily, one, two, or three times, or until the worm is discharged. (Dr. A. Fleming, p. 115.)

Areca Nut.—The areca nut is the kernel of the fruit of the species of palm called *Areca catechu.* It is commonly called the betel nut, being used along with the leaves of the betel pepper for chewing in eastern countries. This nut is a very good remedy for tapeworm. Five or six drachms of the powdered nut should be given stirred up in milk. The worm is usually discharged four or five hours afterwards, alive. The taste is not disagreeable, but is slightly astringent. It is obtainable at very little expense. (Dr. E. Morris, p. 116.)

AFFECTIONS OF THE BONES, JOINTS, ETC.

Amputation at the Hip-joint.—If the patient is not stout, the best plan of stopping hemorrhage is by compressing the abdominal aorta with the tourniquet or compressor devised by Professor Lister, of Glasgow. It is safer than simple manual pressure. It may be necessary to tie the femoral vein. In the case related not a single bad symptom followed the use of the ligature. (Mr. J. Spence, p. 114.)

Bullets in Wounds.—*Nelaton's Porcelain Probe.*—The following instrument was contrived by M.M. Nelaton and Rousseau, in order to diagnose the presence or absence of the ball in Garibaldi's wound. An olive-shaped body made of *unpolished* china was fixed upon the end of a probe, and constituted a simple and most effective instrument for the purpose. Mere contact of the china with lead leaves a mark, proving at once the presence of a leaden body. The mark produced is not obliterated by contact with the soft parts or with morbid secretions. (M. Nelaton, p. 169.)

Favre's Galvanic Probe.—M. Favre has invented a probe by means of which the presence of a bullet or other metallic body in a wound may be easily detected. It consists of a small tube, containing two wires isolated from each other, and terminating in needle points. To the other extremities is affixed a Smee's battery, of a single pair of plates, so feeble as to be unable to produce any sensible amount of electrolysis with the animal tissues. A galvanometer is likewise placed in the course of the intended circuit. If the probe is passed into a wound and comes in contact with a metallic body, of course the current becomes at once established, and is indicated by the galvanometer. The needle points are necessary to pierce any soft tissue interposed between the probe and the bullet. This instrument is extremely ingenious, and is likely to be of use, but cannot be so generally employed in military practice as Nelaton's more simple instrument. (M. Favre, p. 170.)

Chopart's Operation.—Of late years Chopart's operation has been but little practised, Syme's operation, or even amputation of the leg being preferred. The objections to Chopart's operation are these, that the extensor muscles of the ankle having lost their opposing forces, and acting through the tendo-Achillis, draw up the heel, and direct the cicatrix towards the ground, whereby the patient is prevented from walking by the agony induced; that in many cases ulceration and exfoliation of the bone have occurred from tension of the flaps. These objections may be entirely done away with thus: Divide, at the time of operation, the tendo-Achillis near the insertion of the muscular fibres (otherwise the strong fasciæ would prevent proper retraction of the cut tendon), and make the upper and lower flaps as long as they can possibly be made, especially the under one (the upper one should be about an inch long). Thus performed, Chopart's operation is preferable to Syme's, inasmuch as the ankle-joint is left intact, the careful and difficult separation of the soft structures covering the os calcis is avoided, and no bag exists in which matter may, without great care, collect. Pirogoff's operation is superior to Syme's, in that the flap is not hollow but solid, but is liable to the great objection that suppuration along the course of the tendons very generally ensues. Pirogoff's operation will be found fully described in *Retrospect*, vol xxxvii, p. 148. (Mr. H. Hancock, p. 146.)

Excision of the Ankle-Joint.—Excision of the astragalus and os calcis have been performed, but excision of the entire ankle-joint has only been done about six times, all within the last twelve years. This joint is made an exception to others, and when diseased is not excised, but the entire foot is sacrificed. The joint may, however, be totally excised with great success, four out of five cases having been successful. The steps of the operation will be found well described at p. 151, by the author, who is in this country the sole authority upon the subject. (Mr. H. Hancock, p. 150.)

Excision of the Knee-Joint.—Excision of the knee-joint may now be considered an established operation, and one of the greatest improvements in modern surgery. The rules followed by Mr. Butcher, of Dublin, in performing the operation, so as to ensure the greatest attainable success, will be found at p. 137.

Fracture of the Femur.—A case strikingly illustrating the advantage and necessity of perfect immobility of the parts in fractured femur is related by Mr. James, of the Melbourne Hospital. *Nineteen months* after the accident no union of any kind had taken place. The whole limb was put up in a starch bandage, which also fixed the hip, and lime was given internally. In the course of twenty-two weeks firm union had taken place. (Mr. E. M. James, p. 156.)

Hip-Joint Disease.—No surgical disease of a joint begins in other structures than in the synovial membrane (including sub-synovial tissue) and in the bone; and the hip does not differ in this respect from the other articulations. The most important element in the treatment is to obtain complete rest, by enveloping the diseased thigh and pelvis in such a way as to prevent mutual movement. The best apparatus for this purpose consists of a thigh-splint long enough to reach from the crest of the ilium to below the foot. At an obtuse angle to its upper end is fastened a pelvic portion, made of wire gauze strengthened at its edges by stronger wire, or of perforated zinc; this passes round the pelvis just below or on a level with the crest of the ilium from one spinous process to the other; the two ends are connected by an india-rubber cord, passing across the belly, securing the close fit of the pelvic portion. The whole splint is to be bandaged on the limb from the foot upwards. When synovitis is the starting point of the malady, counter irritants, if used at all, must be of the most superficial and slight character. The opposite extreme, of what are called soothing remedies, is equally injurious in chronic cases. Too much poulticing conduces to a loose, flabby, congested state of the parts. A very slight application of the actual cautery is often of the greatest benefit in relieving deep-seated pain. The iron should be a triangular prism, mounted on a crooked stem, so that it may be applied in a linear manner on the skin. (Mr. R. Barwell, p. 139.)

Knee-Joint Diseases.—*Importance of Rest.*—Most of those extraordinary cases of distortion of the knee-joint, following inflammatory disease of the articulation, result from want of mechanical resistance to the muscular traction exercised on the bones. A fixed rigid splint will prevent such deformities, provided it be sufficiently resisting. A gutta percha splint is not strong enough. A firm padded splint of leather, wood, or iron, ought to be placed along the posterior and lateral part of the joint, and it is always advisable that the limb should be swung, as in the case of fractures of the leg. If great contraction have already taken place, it may be necessary to divide one or more tendons of the hamstring muscles. Before doing this, it is well to ascertain the position of the posterior tibial nerve, which may generally be recognised by the pain induced when pressure is made upon it. (Mr. J. Hilton, p. 134.)

Ligatures.—*The Wire Compress, a Substitute for the Ligature.*—The wire compress is a modification of the method of arresting hemorrhage devised by Dr. Simpson, of Edinburgh, and named by him "Acupressure," and is thought to possess the advantages without the disadvantages of that proceeding. The wire is thus applied:—Take a piece of surgical wire six or eight inches long, and thread each end thereof upon a straight needle. Seize the bleeding mouth of the artery with forceps, and pass one of the needles close on each side of the artery just mentioned, about a line above the points of the forceps, directly down through the substance of the flaps, so that they emerge at the cuticular surface, about half an inch distant from each other. Draw them both through together till the curve of the wire compresses the artery on the face of the flap. Now get rid of the needles by clipping through the wire close above their eyes, and also detach the artery forceps. Place a piece of cork, cut for the occasion, upon the skin, between the points of exit of the wire, and over this twist the wire tighter and tighter until the bleeding is arrested. Lastly, cut off the superfluous wire. All which is done quicker than described. Two or more arteries lying near together may be embraced by one wire, and, as has been said, the veins may be included or excluded at will. The finest and softest passive iron-wire is equally as suitable as silver-wire. The needles are about 3 inches in length, straight, and three-edged, with an eye adapted for carrying wire. The wire must be carefully kept from twisting. To withdraw the wire, clip it close to the edge of the piece of cork, and straighten out the curve it has formed at its exit from the skin. Remove the cork, and apply instead the tip of one finger, with which press firmly upon the flap, making traction gently and gradually upon the other end of the wire. For small vessels a few hours of compression is sufficient, and for the largest arteries a much less time than might be supposed. The best plan is to remove the wires from the secondary branches in from twenty-four to forty-eight hours, and from the largest arteries in four days. (Mr. J. Dix, p. 158.)

"This compress has not the slightest advantage over the ligature." The ligature is "one of the grandest things in surgery." The tracks of the ligature act as vents for the discharge of pus, and as regards their preventing healing by the first intention, the advantages of this kind of union are much overrated. (Prof. Fergusson, p. 162.)

Silver-Wire Ligatures.—The effect of a silver-wire ligature upon an artery is precisely identical with that of an ordinary silk ligature. But there is one material advantage in a silver-wire ligature over a silken one, and this consists in the capability of silver-wire to remain in tissues nearly if not quite innocuously. Thus the ligature does not of itself constitute a focus for purulent secretion, permeating tissues which we are endeavouring to unite by immediate adhesion, and these tissues temporarily weakened by the supply of blood being diminished. A silver-wire ligature, which I inserted four years ago in the soft palate of a girl still remains, and she will not allow its removal, as she feels no inconvenience from it. (Mr. R. Davies, p. 164.)

The "Telegraph Suture".—A new form of suture has been lately introduced by Mr. Clover, which appears to possess many advantages over those ordinarily employed, whether of silk, thread, or metal. It consists in the adoption of a very fine copper-wire covered with gutta-percha, in fact, a minute telegraphic wire, and therefore receiving the name of "telegraph sutures." This may be advantageously employed in the form of an interrupted suture in hare-lip, and leaves no no scar compared to that left by the hare-lip pin. This wire is quite soft, and forms a knot like ordinary silk-thread; it can be cut like silk, and be as readily taken out; it can moreover, be used with a fine sewing needle. (Mr. Clover, Mr. Erichsen, p. 163.)

The Horsehair Ligature.—The introduction of wire sutures is easy enough, but the withdrawal is often attended with great inconvenience, and even with risk of tearing the imperfectly united edges of the wound asunder, owing to the rigid condition the wire assumes before its removal. Horsehair, such as is ordinarily sold by fishing-tackle makers, is an excellent substitute for wire, and there is much greater facility in its adjustment and subsequent removal. Both materials are equally unirritant. Horsehair is peculiarly suitable for wounds of the eyelid, and other parts of the face, and to the loose integuments of the scrotum and penis. It is not suitable for wounds where there is much tension between the edges. Long white tail hairs are the best. (Mr. T. Smith, p. 167.)

ULCERS OF THE EXTREMITIES.—*Treatment by "Sealing."*—Entire exclusion of the air from an ulcer whilst healing, is found to assist materially in producing rapid cicatrisation, when once a healthy action is set up. The best manner of excluding the air is the following:—Cover the margin of the ulcer with adhesive soap-plaister, half an inch wide, and over this carefully affix, by means of collodion, a piece of oil-silk large enough to cover both the ulcer and the plaister, and over this again put another edging of plaister. The transparency of the oil-silk allows the progress of the ulcer to be inspected with the greatest ease. As the treatment is proceeded with the discharge gradually diminishes in quantity, hence, though it may be necessary to remove the first sealing on the second day, the second may be left untouched for five or six days. (Mr. B. Holt, p. 220.)

AFFECTIONS OF THE SKIN, ETC.

Burns.—A most excellent application is a paste composed of common whiting, olive oil, and vinegar. This should be spread on strips of rag. It produces a marked sedative effect, no doubt owing to the escaping carbonic acid set free by the vinegar. (p. 224.)

Elephantiasis of the Leg.—The best and perhaps only successful plan of treatment is by cutting off the main arterial supply of blood to the limb, by tying the femoral artery at the apex of Scarpa's triangle. In some cases it will suffice to tie the popliteal, and this should be preferred if the case admits of it. (Dr. Carnocan, Mr. R. Davies, p. 164.)

Ringworm.—Take two drachms of pure iodine, and rub down with it, in one ounce and a half of spirits of wine, as much of the pure hydriodate of potash as will dissolve it. Paint the parts affected with this by means of a camel-hair pencil every two hours until they have become quite black, extending the application one quarter of an inch over the edge of the affected parts. The skin comes off in large flakes. Continue the application until a perfectly new skin is formed. (Dr. A. Lane, p. 217.)

AFFECTIONS OF THE URINARY ORGANS.

Albuminuria.—*Arsenic.*—A case of albuminuria is related in which all other remedies being unsuccessful, Fowler's solution of arsenic was given "with the most marked and decided advantage." The patient "began to improve in less than a week, his spirits began to return, his appetite to improve, and the albumen to disappear from the urine." On omitting the arsenic the albumen reappeared, but the case was ultimately cured under its use. We suppose its effect must be ascribed to the wonderful tonic properties of arsenic in some cases attended by nervous exhaustion and muscular atony—the cure of the local affection resulting from the cure of the abnormal constitutional state. (Mr. J. Wearne, p. 131.)

Diabetes.—Do not suppose that every case in which there is saccharine urine, is a case of diabetes in the ordinary acceptation of the term, to be treated on an uniform plan dietetically and medicinally. It is now pretty well determined that mental irritation by its action on the brain may give rise to saccharine urine, and such cases must be treated on a broader view than would suggest itself by regarding them simply as cases of ordinary diabetes. Should such a case occur to you, pay little attention to the urine, and advise detachment from harassing occupation and thoughts, change of air and recreative amusement, with such diet as is best calculated to restore the integrity of the digestive functions. This is the only rational and successful plan of treating such cases. (Dr. D. Noble, p. 119.)

IRRITATION OF THE BLADDER.—*Triticum Repens.*—In cases of constant and severe irritation of the bladder from any cause, an infusion of the underground stem of the *Triticum Repens* (dog's grass, or couch grass) is an agent of much use. It is of much value in cases of renal calculus. It lessens frequency and pain of micturition, affording relief when buchu, pareira, uva ursi, and other infusions fail. An ounce should be infused in a pint of boiling water for an hour, and the strained liquid be taken when cool, during a period of twenty-four hours. It may be used as a vehicle for other agents. It is somewhat diuretic, and perhaps slightly aperient. (Mr. H. Thompson, p. 132.)

RETENTION OF URINE.—Serious errors are sometimes committed, both in the diagnosis and treatment of cases of retention of urine not dependent upon stricture of the urethra, and in which that canal is in a healthy condition. Retention of urine in a healthy man may be said to occur generally from one of three causes: 1st, from over-distention consequent on the postponement of the act of micturition; 2nd, upon spasm arising from irritation of the urine after a debauch; 3rd, from either of the above causes acting upon an elderly person, subject to more or less enlargement of the prostate. In cases where there is doubt as to the existence of retention, the passage of a catheter ought to clear up the difficulty. But here a serious error occurs sometimes, either the instrument never enters the bladder from being too short, or from being thrust onwards and making a false passage in which the point becomes entangled. An elastic catheter may bend upon itself. The only way to be perfectly certain that a catheter has reached an empty bladder, is to inject water through it. In some cases, in addition to the urgent symptoms consequent upon an over-distended bladder, there is considerable hemorrhage—nothing arrests this like turpentine in ten minim doses. If the catheter becomes blocked with clot, inject a little water through it, and after hemorrhage it is a good plan to inject the bladder with cold water; if there is not expulsive power, the urine must be withdrawn again in a few hours and the catheter left in. All cases of this nature are owing to paralysis of the bladder, consequent on over-distension. The surgeon attempts to pass a catheter, but failing, puts down the cases as examples of suppression. The diagnosis between retention and suppression is so very clear as to render a mistake perfectly inexcusable. In suppression there is an absence of that violent desire to micturate so characteristic of retention, and of the solid tumour above the pubes formed by the distended bladder, and the patient lies in a listless condition soon passing into coma, whilst the breath and skin exhale a strong urinous smell. (Mr. B. Holt, p. 192.)

STRICTURE OF THE URETHRA.—*Treatment by Gradual Distension.*—It is well known that when any fibrous structure is *over*-distended its tendency to contract is destroyed. This happens in the case both of healthy and morbid fibrous tissues. Now, if a stricture of the urethra can be over-distended, we have a means of ensuring its non-recontraction. This has been accomplished by Mr. Henry Thompson. The instrument used by him consists of two long and narrow steel rods, accurately applied throughout their entire length by the single plane surface which each possesses. These rods are united at both extremities, and are surmounted by a handle resembling that of an ordinary sound. This handle is attached to a screw with a very fine thread, which being turned, causes the two rods to diverge very slowly and very gradually from each other, at a given spot, about six inches from the handle, and the object in the use of the instrument is to place this portion of its length in the stricture, which may then be dilated to any size desired, the rest of the urethra not being affected. After the removal of the instrument all that remains to be done is, to pass a full-sized metallic instrument every second day for a week, and after that at increasing intervals for a week. (Mr. H. Thompson, p. 201.)

Immediate Treatment.—Smyly's Dilator.—Dr. Smyly, of the Meath Hospital, has invented an instrument for the "immediate" treatment of stricture of the urethra essentially similar to Mr. Barnard Holt's, but differing in this, that a guide of silver wire, having upon it a No. 1 catheter is first passed; the catheter (which is perforated at the point) is then withdrawn, and a dilator similar to Holt's is then passed, and the stricture forcibly expanded. The chief advantage of this plan is, that it is not necessary to pass first thing a dilator equal to a No. 3 or No. 4 catheter, without any guide whatever—a thing difficult to accomplish and sometimes dangerous to attempt. (Dr. P. C. Smyly, p. 199.)

VENEREAL AFFECTIONS.

Gonorrhœa.—*Injections.*—The main treatment of gonorrhœa should be by injections, copaiva and cubebs being seldom necessary. The rule for their use is simple: If the inflammation be severe, let the solution be weak and frequently used; if it be of chronic type, let the solution be strong and seldom used. This rule is not only applicable to the treatment of gonorrhœa, but it may be extended (with necessary modification) to treatment of all inflammations, and to the use of internal remedies, as well as of external applications. There is a general law regulating the actions of a large class of remedial bodies. The terms of it would run something in this form: "The more acute the disease, the more frequent and the weaker the remedy; the more chronic, the stronger and less frequently applied." One of the most reliable astringents for the cure of gonorrhœa is alum. In the most acute form of gonorrhœa, when the discharge is profuse, thick, and glutinous—the lips of the urethra red, villous, and pouting—the patient should be directed to pour a small jug of cold water over the organ, and immediately inject a syringeful of solution of alum of the strength of half-a-grain to the ounce. This injection should be repeated every half-hour for the first day, and as often at night as the intervals of sleep will allow. Along with this a saline purgative, and tartar emetic in minute doses, should be given. Next day the injection may be doubled in strength, and used every hour. Probably in forty-eight hours more the discharge will have ceased entirely, the injections must, however, be continued a week or so, of the strength of half-a-drachm to the eight ounces, three times a day; otherwise a relapse may occur, which will be harder to cure than the original clap.—(Dr. M. H. Collis, p. 225.)

Gonorrhœal Ophthalmia.—This formidable affection yields with mavellous rapidity to repeated weak injections. In the most acute cases a solution of a quarter of a grain of nitrate of silver to the ounce of distilled water should be injected beneath the upper lid, with a syringe, every ten minutes for the first hour; after that a half-grain solution should be injected every half-hour. If this is carefully carried out for the first twenty-four hours, the patient's eye will be quite safe. A stronger solution may then be used, but less frequently, and in a couple of days, if the villous condition of the conjunctiva should seem to require it, Guthrie's ointment of nitrate of silver may be used. These weak solutions are quite useless unless very frequently applied; but if so used, scarcely an eye need be lost from gonorrhœal ophthalmia.—(Dr. M. H. Collis, p. 257.)

GONORRHŒAL RHEUMATISM.—Gonorrhœal rheumatism is due to the action of a specific poison, of a vaginal origin, and probably distinct from that which occasions ordinary gonorrhœa. Its treatment is very unsettled and unsatisfactory. There is only one remedy which has any decided action for good, and that is biniodide of mercury. The following may be administered three times daily: Solution of bichloride of mercury, two drachms (half a grain of the salt); iodide of potassium, half a scruple; cinchona draught, ten drachms. When the affection of the joints secondary to gonorrhœa has existed for many months, thickening of the ligamentous and other structures around the joints ensues, and is liable to produce permanent disability of the joints. There is only one plan of successfully treating this state, and that is, by the external use of iodine, and friction by the hand. The cold douche should first be applied to the joint, then active friction for some minutes; and lastly, lint steeped in a weak solution of iodine should be laid on the part—this should be covered with flannel and left on until the next morning, when the process should be repeated. (Dr. H. W. Fuller, p. 34.)

NODES.—*Biniodide of Mercury.*—Biniodide of mercury is upon the whole more useful in cases of syphilitic nodes than is even the iodide of potassium. Biniodide of mercury is a scarlet powder, insoluble in water, but soluble when iodide of potassium is present likewise. Hence the best plan of administration is to form it extemporaneously, by the addition of iodide of potassium to the bichloride of mercury. Made thus, much reliance may be placed on its action, and its operation can be regulated with the greatest nicety. When given in powder it is very uncertain in its action. (Dr. H. W. Fuller, p. 33.)

SYPHILIS.—Absorbent medicines, such as mercury and iodide of potassium have no real curative power over the syphilitic poison itself, but are of value in direct proportion to the formation of low organisable material. Hence the failure of the iodide in cases where simple rashes on the skin exist, and its efficacy where there are nodes and other symptoms of inflammation of fibrous tissue. (Dr. Wilks, p. 235.)

SYPHILITIC SORE THROAT.—*Chlorate of Potash.*—In a case of aggravated syphilitic throat affection, in which mercury in various forms, and iodide of potassium had been tried, and nitrate of silver applied locally, without success, chlorate of potash soon effected a perfect and permanent cure. (Mr. Dryland, p. 226.)

AFFECTIONS OF THE EYE AND EAR.

CATARACT.—*Inutility of the Candle Test.*—What is well known as the candle test is not at all reliable as a means of diagnosis in cataract. The three images may be seen in an eye which presents a very observable cataract. (Mr. E. Hart, p. 236.)

To Test the Catoptric State of the Lens.—The pupil being dilated with atropine, place the patient in a darkened chamber. Holding a candle in front of the eye, and interposing a large lens, by successive movements the light may be focussed upon any part of the surface or depth of the crystalline lens and its membranes, and the whole may be so accurately lighted up, that the slightest deposit or opacity of any kind is readily discovered. (Mr. E. Hart, p. 237.)

Is there Amaurosis likewise?—Let the surgeon gently press the ball of his eye with the tip of the finger, on the nasal and then on the temporal side of the ball of the patient's eye, and what are called the retinal phosphenes will appear to the patient. These are not seen in an unhealthy or amaurotic state of the retina. Hence, if in a case of cataract, they can be made to appear, or not, we have a good diagnostic sign on which to form an opinion as to the utility or inutility of an operation. (Mr. E. Hart, p. 238.)

ENTROPIUM.—In one class of cases the inversion of the lids is due to the change in the lid itself, in the other class it is owing to spasm of the orbicularis muscle. One form of the latter class of cases is almost entirely seen in elderly people with loose superabundant folds of skin about the lids; it is limited to the lower lid, and takes its origin in an attack of ophthalmia. The ciliæ lie buried down in the palpebral fossa quite out of sight. The irritation is so inconsiderable, that in most instances it is better not to interfere with the case. When the affection is owing to real organic change in the lid, the most effectual cure is to pinch up and remove a small portion of loose skin close to the margin of the lid, and then to remove a corresponding portion of the orbicularis muscle; this is a very essential part of the operation. A little judgment is required to remove sufficient to ensure a normal position of the lid. A very serious form of entropium results from a gradual thickening and contraction of the inner edge and surface of the tarsal cartilage, curling inwards. This is a slow morbid change, depending upon chronic ophthalmia of the catarrhal type. The best plan of treating this inveterate form of disease is the following: The patient being placed under the influence of chloroform, a bone spatula, suitably shaped to fit the convexity of the eye and the concavity of the lid, is passed up between the two, and is held by an assistant in such a way as to raise the lid from the eye and to keep the lower edge well upon the stretch. An incision is now made along the entire edge of the cartilage, just within the roots of the eyelashes. This is carried back so as raise the lashes with their bulbs from the surface of the tarsal cartilage. This requires a separation of the soft parts from the lid to the extent of about three lines. The next step is to remove with a pair of scissors all that portion of skin, including the bulbs of the lashes, that has been previously dissected up. The loose skin of the lid may then be allowed to fold over the wound and the operation is complete. Great care is required not to leave any of the bulbs behind, otherwise the object of the operation is partially frustrated. It very speedily heals, with only a fine line of cicatrix at the tarsal margin. (Mr. G. Critchett, p. 253.)

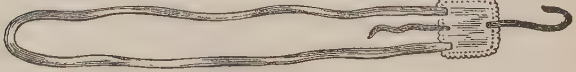
SCLEROTITIS AND IRITIS.—The plan of internal administration of morphia, in acute cases of sclerotitis and iritis, continues to be as successfully pursued as hitherto. At the commencement of a case doses of $\frac{1}{4}$ gr. should be given, cautiously increased to $\frac{1}{3}$ gr., or even $\frac{1}{2}$ gr., but in some young or feeble subjects it is well to begin with $\frac{1}{5}$ gr. The patient should be directed to take one dose every third hour; but on the pain becoming less, to increase the interval of the doses to four or five hours, or even to leave off the medicine altogether. The degree of pain should be the guide as to the length of interval. A mercurial purge and tonics may be required at the termination of the case. In a few cases morphia causes violent symptoms of stomach derangement and depression; in others it produces these effects but in a slight degree; in such cases the drug is inadmissible, except in very small doses. Where there is iritis, belladonna should be applied locally. The important practical fact seems established, that morphia is, *per se*, a powerful antiphlogistic, capable of curing those acute inflammations of the eye in which up to the present time, blood-letting, blistering, and mercurialisation have been considered necessary. The explanation of this remarkable action of morphia in reducing abnormal fulness of the vessels of the sclerotic, we may find in the relations of pain to vascular congestion. It is generally considered that the pain is the effect of congestion, but it is quite an open question whether, in certain classes of cases, the order of things may not be reversed. (Dr. J. Z. Lawrence, p. 245.)

STRABISMUS.—*Von Graefe's Operation.*—The principle of Von Graefe's operation consists in a very careful division of the tendon of the muscle close to its insertion, with the smallest possible amount of laceration of the subconjunctival tissue. Our object is only to weaken the muscle by giving it a more backward insertion, and not to render it more or less impotent. The steps of the operation are these: Place the patient under chloroform, keep the eyelids apart by the spring speculum, or, if this proves not sufficiently strong, by the broad silver elevators; let an assistant evert the eye with a pair of forceps, seize with a pair of finely-pointed forceps a small but deep fold of the conjunctiva and subconjunctival tissue, close to the edge of the cornea, and about midway between the centre and lower edge of insertion of the internal rectus; snip this fold with the scissors, (which should be bent on the flat and have tolerably sharp points), and immediately divide the subconjunctival tissue in a downward and inward direction. Now pass the squint hook through the opening to the lower edge of the tendon, and pressing its point somewhat firmly against the sclerotic, slide it upwards beneath the tendon, as close to its insertion as possible, and catching up the whole expanse of the tendon. Now gently push off with the points of the scissors the conjunctiva which covers the tendon and hook, and carefully snip through the tendon as close to its insertion as possible. It is well now, by means of a smaller hook, to ascertain that all the lateral fibres of the muscle are divided, as sometimes the lateral expansions of the tendon are considerable. The after-treatment is simple, consisting chiefly in the diligent use of cold-water dressing. (Dr. Soelberg Wells, p. 238.)

TRICHIASIS.—By trichiasis is meant an alteration in the growth of one or more of the eyelashes, in such a way and to such an extent that the surface of the eyeball is swept over. Sometimes a single fine hair grows in and sweeps the globe, sometimes an extra row of strong well-marked lashes grow from the inner edge of the tarsus direct upon the eye. Where there are only one or two hairs they are very easily overlooked. The palliative treatment of these cases consists in simply pulling out the peccant lash or lashes. For this purpose a particular form of broad-pointed forceps very carefully made is required. In slight cases it is as well to rest satisfied with this simple and mild treatment, as there is so much difficulty in effecting a radical cure. Various means have been suggested for accomplishing a radical cure, but they all occasionally fail, and none are suited to all cases. It has been recommended to remove the lashes and insert a fine cautery into the hair follicle, but when the hair is removed, unfortunately no aperture can be discovered into which it can be introduced. Dissecting lashes out is extremely difficult, and very liable to alter the growth of others. If the patient is bent upon having something tried, the removal of a small triangular piece of the entire lid, involving the part from which the inverted lash grows, is upon the whole the least objectionable plan. The divided parts may be accurately adjusted without any subsequent deformity. Where the inward tendency is only just sufficient to permit the ends of the hairs to touch the eye, a new cicatrix may be formed by removing a portion of the subcutaneous tissue and muscle, close to the root of the eyelashes, so that by the contraction of the cicatrix the direction of their growth may be changed. Very severe cases must be treated as cases of entropium. (Mr. G. Critchett, p. 251.)

MIDWIFERY, AND THE DISEASES OF WOMEN AND CHILDREN.

Congenital Rupture at the Groin.—Steel, wire, or any kind of metallic truss is seldom suitable to young children. The pressure and friction produced can seldom be borne. The following simple bandage will be found to answer all purposes. It consists of a long narrow loop of the finest flannel, rolled upon itself, with a pad of the same material at one end; attached to the pad are two little tape strings. The whole is represented by the following sketch:—



When worn, the circular end of the loop is passed over the thigh of the affected side, and the opposite, or padded end, is carried round the child's loins and opposite hip, so as to encircle the body. The pad is then fitted upon the seat of rupture, and secured underneath the loop around the thigh by the little tape strings. The mother should be provided with three or four of these little supports, so that every day, if necessary, a clean one can be used. (Mr. C. Vines, p. 185.)

CYSTIC TUMOUR OF THE VAGINA.—The following treatment is effective in all varieties of vaginal cystic tumours. The patient being placed on her back, and the vagina opened by the duck-bill speculum on either side, seize the cyst by the vulsellum and puncture it, and then remove by the scissors an elliptical piece of cyst-wall. Lastly, plug the remains of the cavity with lint, soaked in tincture of iodine. The remains of the cyst-wall will gradually shrivel, until hardly a trace will be left. (Dr. R. Barnes, p. 261.)

HYPERTROPHIC ENLARGEMENT OF THE CERVIX UTERI.—*Amputation.*—In a case which lately occurred at the London Hospital, cauterisation was quite unsuccessful, and the following procedure was adopted with great success. The patient being placed under chloroform, the uterus was pressed downwards by firm pressure over the abdomen, whilst the cervix was seized and drawn forwards by Museux's vulsellum, the vagina being opened by a duck-bill speculum on either side. The anterior and posterior lips of the hypertrophied cervix were then successfully cut off by bent scissors. An even incised surface was then secured by paring off the mucous membrane at the angles of union of the lips. Very little bleeding occurred. A small roll of lint, soaked in solution of perchloride of iron, was placed in the canal of the cervix. The stump was then covered by bringing together the edges of the mucous membrane by iron wire carried by the curved tubular needle. In this manner a very neat and perfectly covered stump was made. At the end of ten days the cure was essentially complete, and the os uteri assumed in time quite a normal appearance. (Dr. R. Barnes, p. 344.)

LACERATION OF PERINEUM IN FORCEPS CASES.—*Prevention of.*—The adoption of the following plan in forceps cases obviates all danger of lacerating the perineum. When the forceps have been applied, and the head of the child has been brought down sufficiently low on the perineum that the forefinger of the accoucheur introduced into the rectum can reach, for instance, in occipito-anterior presentations, beyond the frontal eminence, the instrument should be withdrawn, or, if that be not convenient, all traction should be discontinued, and the finger, still retained in the rectum, should be used after the manner of the vectis, pressure being made with it, first on the space between the frontal eminences and the root of the nose, and afterwards on the upper jaw, and perhaps, if necessary, on the chin, as these parts successively come within reach. By this means the head is made to rotate in the axis of the outlet, and, being directed forwards under the pubes, its pressure on the perineum is greatly relieved. (Dr. C. Ricketts, p. 275.)

LITHOTOMY IN THE FEMALE.—It is almost impossible to extract very large stones by dilatation of the female urethra, and moreover, permanent inability to retain the urine sometimes results. Since the improved methods of closing vesico-vaginal fistulæ have come into practice, we may with safety extract such stones by an incision in the vesico-vaginal septum. Bozeman's speculum should first be introduced into the vagina, and the incision must be made upon a grooved staff, passed into the bladder. The closure of the wound should be effected by metallic sutures. (Mr. J. R. Lane, p. 210.)

UTERINE CONTRACTION.—*Ergot of Rye.*—It appears singular that in some cases a specimen of ergot induces good uterine contraction, whilst in other cases it has no effect whatever. The administration of ipecacuanha also induces uterine contraction; and it seems very probable, but requires confirmation by more observations on the subject, that ergot of rye only induces contraction of the uterus when it induces vomiting or nausea. (Dr. G. Hewitt, p. 262.)

VESICO-VAGINAL FISTULA.—In operating for the cure of vesico-vaginal fistula, it will be found that fine strong silk answers even better than wire. It is passed much more easily and with finer needles, causes less irritation of the neighbouring parts, and is removed much more easily, while union takes place quite as well as when wire is used. The use of the catheter may be dispensed with altogether. It is a most troublesome part of the after-treatment, and "I believe," it need not even be used to empty the bladder; moreover, the use of the catheter is not of itself quite free from danger, sometimes producing a species of catarrh of the bladder, persisting for months after the cure of the fistula. (Mr. T. Spencer Wells, p. 291.)

In an aggravated case of vesico-uterine fistula, which occurred at St. Mary's Hospital, as the patient was of the age of forty-five, a proceeding recommended by Jobert was adopted, viz., the closure of the os uteri. The lips of the os were carefully pared and brought together by means of four silver-wire sutures. The ends of the wire were passed through holes in a thin leaden plate, and fastened with perforated shot, after Bozeman's method. The plan was perfectly successful. Of course there remained the communication between the bladder and the uterus as before, but this proved no inconvenience, and moreover served as an outlet for the menses which had not quite stopped. (Mr. J. Lane, p. 283.)

MISCELLANEA.

Atropia and Strychnia.—*Internal Exhibition.*—The internal exhibition of solutions of atropia and strychnia is much to be preferred to the use of the galenical preparations of belladonna and nux vomica, because of their greater safety and efficiency. The solutions used must be so proportioned in strength, that ten minims (by measure) is the ordinary commencing dose, which admits easily of increase for the adult, and of lowering for the child. The solution of *Atropia* should be made thus:—Atropia, 1 grain; distilled water, 5 drachms. Dissolve thoroughly with the aid of a few drops of diluted muriatic acid, and add of rectified spirits sufficient to make 10 drachms. Of this solution 10 minims, contains 1-60th of a grain of atropia. This dose should be given in a little water on an empty stomach, once daily, and may be increased daily by 2 or 4 minims until a slight degree of the early physiological effects is produced. For children of one year and under, the, commencing dose is one minim, of 2 years two minims, and so on. Strong persons require larger doses than the above. It should be given only once daily, because the action of one dose does not subside completely for sixteen or eighteen hours. Unless given on an empty stomach it is absorbed too slowly, and the effects of the drug are manifested very imperfectly or not at all, because it is eliminated by the kidneys almost as quickly as absorbed. Atropia should never be given in pill, which may undergo solution very slowly or not at all. The solution of *Strychnia* should be made thus:—Strychnia, 2 grains; distilled water 5 drachms. Dissolve the strychnia thoroughly with the help of a little diluted muriatic acid, and add of rectified spirit sufficient to make 10 drachms. The dose of ten minims contains 1-30th of a grain of strychnia. When employed for its *tetanic* action the solution should be taken in the morning, half an hour before breakfast, and in half an ounce of water, and the dose increased 2 or 4 minims daily, until a slight degree of the physiological action is manifested, when no further increase should be made. Strychnia ought never to be given in pill. It is hard of solution in the weak acids of the stomach, and several pills may remain unchanged and accumulated there or in the bowels. A change in the secretions may then dissolve and transport them all simultaneously into the blood, and give rise unexpectedly to alarming symptoms. This is the explanation of the so-called cumulative action of strychnia. (Dr. A. Fleming, p. 374.)

CASTOR OIL.—*Mode of Administering.*—The following is a pleasant mode of administering castor oil. Put the quantity of castor oil required into an earthern pan, and break an egg upon it. Then let the pan be heated and the oil and egg shaken up together so as to produce an *œuf brouillé.* A little salt must then be added. A person would eat an omelette of this kind without finding out the presence of castor oil. (M. Martin, *British Med. Journal, Nov.* 22, 1862, *p.* 538.)

DIGITALIS.—The action of digitalis in producing a tonic condition of the organic muscular fibres, is not confined to the heart, for if given in full doses it induces violent uterine contraction and checks uterine hemorrhage; and inasmuch as its action in staying menorrhagia and uterine hemorrhage is permanent, it seems fair to conclude that it gives tone to the capillaries, and increases their contractility. (Dr. Dickinson, *Med. Chir. Trans.*, vol. xxxix.)

LUNAR CAUSTIC.—*A new Mode of Preparing.*—The double nitrate of silver and soda, forms, when cast into a mould, a pencil having different properties to the ordinary lunar caustic. Its action is less that of a cauteriser, and it is hence much more suitable for application to delicate surfaces, such as the conjunctiva. It is prepared thus: Dissolve in distilled water one part of nitrate of silver, and in another portion of water one part of nitrate of soda. Mix, and then evaporate; lastly, fuse in a crucible, and pour into a heated and greased mould. (M. Bron, *Med. Cir.*, Oct. 15, 1862, p. 272.)

ORGANIC POISONS.—*Alkaline Permanganates.*—The alkaline permanganates are calculated in an eminent degree to meet every case of poisoning by organic poisons, such as aconite, strychnine, opium, &c. Condy's fluid consists of combinations of these salts in solution, and hence they may always be procured without any difficulty. The action of permanganates on poisons of organic nature is readily explained chemically. [The above is solely the result of reasoning from a knowledge of the chemical relations of the permanganates, and requires and deserves actual trial for confirmation.] (Med. Chir., p. 371.)

POISONING WITH MORPHIA.—*Caffeine.*—A case of poisoning by morphia is related by Dr. Anstie, of Chelsea, in which caffeine was administered. "The effect was remarkable; the pulse, which had been scarcely perceptible, rose to a fair strength." This improvement, however, did not continue, and the patient gradually sank away. (*Med. Times and Gazette, Feb.* 7, 1863, *p.* 134.)

QUININE.—*Therapeutical Action.*—Quinine is held to be a direct tonic on insufficient grounds, its real action is that of a sedative to the efferent nerves of the sympathetic, for the following reasons: That it requires a very much larger dose of quinine to produce its physiological effects in robust persons than in the debilitated; and in extreme asthenia, even small doses are poisonous. That it decreases the force and frequency of the pulse. When given in inflammatory fever, it restores the activity of the secretions. The following practical points will be found true. Quinine is only useful in dyspepsia which results from suppression of the digestive secretions from febrile irritation. It should be used rather in sthenic than asthenic cases of inflammation. It is not suitable in diseases of the heart. (Mr. R. Walker, *Med. Times and Gazette, Feb.* 21, 1863, *p.* 203.)

STRYCHNINE.—*Tannin as an Antidote for.*—If administered in time tannin is an excellent antidote in cases of poisoning by strychnine. It is necessary to give twenty to twenty-five times as much of the antidote as of the poison, and it would be prudent to give much more. An infusion of black tea even is efficacious if the dose of strychnine taken is small. Oak bark may generally be procured, it contains about eight per cent. of tannin, and may be given as decoction. (M. Kurzak, p. 376.)

Commentary on Midwifery,

AND THE

DISEASES OF WOMEN AND CHILDREN.

FOR THE LAST HALF-YEAR.

We have again to present to our readers a few remarks on what has occurred in this department of Medicine during the last six months. We have only seized on the chief and most interesting topics of discussion in the different medical periodicals of the time, the less interesting and more common affairs of this department have not occupied our pages, and consequently we have omitted them here.

There is no doubt that Abortions in the early months of pregnancy are greatly under-estimated. Dr. Whitehead, of Manchester, seems to prove that this is the case. It is probable that abortions, including those of one, two, and three months, are as numerous as all the births at the full period of pregnancy. The female is often supposed not to have aborted, but to have suffered from some irregularity of menstruation. Before marriage she was healthy, but after marriage she is always ill, and every month or two her womb manifests some of the unpleasant disturbances of menstruation, but as she has only passed a week or two over her usual period it is looked upon as nothing. She has probably, however, aborted, and goes on in this way month after month and year after year. "Healthy before marriage, she has not been well as a wife; she has no offspring. It is not uncommon with her friends, under these circumstances, to attribute her alleged indisposition to disappointment. Such a person will probably state, that soon after marriage, her monthly visitations, regular and comfortable previously, have become changed in character or periodicity, and are attended with suffering. Sometimes delayed, occasionally too frequent, the product is unusual—fibrinous or membranous, broken, or grumous; now and then offensive; and each monthly crisis constitutes a paroxysm of serious illness. She loses flesh, and strength, and mental elasticity, and is chided for being mopish. Another, since marriage, has had the menstrual function delayed to the fifth or sixth week, or longer, when a hemorrhagic change takes place, which is looked upon as an abortion, and probably was so; and for years afterwards she has periodical metrorrhagia or other form of irregularity, with distressful suffering, but no pregnancy,—or, at least, no offspring. In other cases, to the birth and comfortable nurture of one or several children, succeeds one or more abortions, then menorrhagia, with morbid product and dysmenorrhœa, anæmic debility, and, sooner or later, some form of organic mischief—commonly of the lungs or heart, and all without any clearly assignable first cause. It is by no means uncommon to hear a woman, thus or similarly circumstanced, declare that she has miscarried six, or eight, or ten times in the space of one year; but her statement is seldom credited in this sense. The phenomena she describes are commonly classed under the vague denomination of dysmenorrhœa or hysteria, without reference to cause. It is not improbable, however, that the patient's own interpretation was the correct one; for, under the various forms of morbid irritability of the uterus and its annexes, conditions of daily frequency, that special and all-important process which nature sets up consentaneously with conception, for the purpose of forming and organising the primary envelope, being frustrated in the onset, the fertilized ovum, no adequate means being provided for its reception and accommodation, is thrown off soon after its arrival in the uterine cavity. There can be no room to doubt that accidents of this kind are much more frequent than is commonly supposed or believed. Thus, the highest rate of mortality of the human product takes place, in all probability, during the tenderest state and earliest stage of its existence. It is scarcely questionable, indeed, that abortions are, by far the most common during the first three months of pregnancy, and especially during the first third of this period; but owing partly to the minuteness of the object voided, sometimes to its partial dissolution, or shrinking into a shapeless, shreddy mass, and, perhaps, also to the familiar frequency of the occurrence, the embryo is suffered to pass away unnoticed and unsought for." "Thus it will appear, that for every premature birth during the last three months of pregnancy, there are four abortions during the preceding three months. And taking it for granted that the number of such events during the first three months exceeds those of the second in the same ratio as the latter do those of the third—which is by no means improbable—then the relative frequency of immature births in the first, second, and third epochs of pregnancy of three months each, will stand in the progressive order of 16, 4, 1."* These are important considerations in the treatment of such cases. The deciduous membrane in such cases, is out of all proportion to the size of the ovum. The ovum may have perished soon after arriving in the womb, or even before doing so, but we believe that the deciduous membrane will sometimes continue to be developed, and give rise to much more trouble than if it had perished at the same time as the ovum itself.

The following case of Induction of Premature Labour illustrates what can be done in order to dilate the os uteri in cases of extreme need. It will be seen that not only was Dr. Barnes' method of dilating the os adopted, but *incisions* were made in the os, and dilatation again followed; so that these incisions must have been greatly extended by tearing them during the after process of dilatation. If this practice had not been adopted by so eminent a man as Dr. Barnes, we confess that we should have been afraid to make use of it, although we well remember a case of our own, in which, when we had the forefinger on the anterior lip of the os, a violent pain came on and tore a rent in the os uteri which seemed to be one or two inches long. Yet the woman did well, and never knew of the injury which the child had inflicted upon her. It seems that rather severe lacerations and injuries may be inflicted on the os uteri, provided the peritoneal sac remains intact. The following is Dr. Barnes' case:—"A tactile examination reveals a scirrhus-like induration of the cervix uteri, which is nodulated. One very hard nodule, larger than a hazel-nut, occupies the right side of the anterior lip and cervix; there are several smaller ones. The catamenia ceased on the first of May last: and as she was so reduced by the vomiting, Dr. Barnes, at her own earnest request, appointed Christmas-eve, at seven p.m., to induce premature labour, remarking at the time that it would not interfere with Christmas-day, as he could promise to terminate the labour in less than eight hours. It was all over in half that time, as the sequel will show. At fifteen minutes past seven on that evening Dr. Barnes introduced a small caoutchouc dilator into the os uteri by means of the uterine sound. It would then barely admit the tip of the little finger. In half an hour, by water pressure, two fingers could be admitted, and the vagina was nicely dilated. The stop-cock was then turned off, and the instrument removed for a little while. A rather larger dilator being then introduced, another half hour had enlarged the os uteri to admit nearly three fingers, but dilatation went on very slowly in consequence of the scirrhus-like induration. Dr Barnes then ruptured the membranes at half-past eight p.m., and incised the os uteri at several points with a guarded penknife. The morbid thickening and rigidity of the cervix were extreme, and would have been incompatible with the birth of a living child at term. Some pains followed, and the dilator was again introduced, and pretty fully distended with water. After about half an hour more the dilator was removed, and opportunity given to allow the induced uterine action to work. A little before ten p.m. the os uteri was sufficiently dilated to allow Dr. Barnes to adapt the long forceps, with which, in another half hour, he terminated the labour. The face was directly anteriorly. The placenta followed in a few minutes, and the uterus contracted well. The child, a boy of good size for eight months, was born semi-asphyxiated; but after half an hour's use of warm and cold water affusions, and Dr. Silvester's method of artificial respiration, it respired freely. The mother has not had a single bad symptom, and the child, though feeble, bids fair to live. Dr. Barnes hopes that besides relieving the vomiting, (which it has done,) this premature delivery may, by the mammary diversion keeping the uterine and ovarian organs in quiescence, afford opportunity for recruiting the mother's general health."*

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* Medical Times and Gazette, Dec. 20, 1862, p. 654.

Dr. Parker maintains that Tartarized Antimony has a decided effect on the womb, similar to ergot of rye; and Dr. Graily Hewitt says the same of Ipecacuanha. Although we differ from both these gentlemen, yet we are bound to pay attention to such respectable authorities. The act of vomiting no doubt appears, in some labours, to increase the action of the womb, as well as to cause a relaxation of the lower fibres of the organ; and it seems to be a law, that as the lower parts of the womb relax and dilate, the upper parts contract, the same as in nearly all organs of extromission, such as the rectum and sphincter ani. Yet we doubt the accuracy of Dr. Parker's and Dr. Hewitt's explanation of these drugs. There is no doubt that both these emetics are valuable in simulating the sickness of the early stages of many labours, and thus relaxing the surrounding tissues, but nature generally stops this nauseating process as the labour advances, and instead of relaxing, she puts forth all her powers to produce the most powerful muscular contraction in the fibres of the upper parts of the womb. It would be all very well, in giving antimony or ipecacuanha, if we could confine their effects to relaxing the fibres at the outlet of the womb, and round the vagina and perineum, but we think that these medicines, whilst they acted thus, would also act similarly on the muscular fibres at the fundus

* Lancet, Jan. 3, 1863, p. 10.

uteri. It is hardly likely that these medicines can act so distinctively as to relax one set of muscular fibres, and at the same time contract the other. It is possible that both Dr. Parker and Dr. Hewitt have made the following mistake in their explanations:—The simple operation of the finger in dilating any sphincter will dispose the muscular fibres higher up in the canal to contract; for example, if we dilate the sphincter ani with the finger or a bit of soap in a child, or even in an adult, the fibres of the rectum, and even of the pelvic and abdominal muscles are called into action for the expected process of defecation. In the same way, if we dilate the os uteri with the fingers, we often cause the muscles higher up the womb to obey the summons which the os gives them to contract. Nearly all muscles at the outlets or mouths of tubes have this power to call upon their colleagues higher up to assist them in their work. In this way, nauseants may probably so relax the outlet of the womb and vagina, as to call upon the muscles at the fundus uteri to bestir themselves; but we think it very doubtful whether they possess any direct effect, as Dr. Parker and Dr. Hewitt say, in causing contraction of these muscles. Dr. Parker, however, says, "*It stimulates to increased contractile action the oblique, longitudinal, and other muscular fibres concerned in expelling the child.* It may be said that this increased contractile action is only apparent, and that the advance of the labour is due to the decreased rigidity and resistance of the os and more external parts, in consequence of the tartarized antimony having produced the results stated in the two preceding propositions. This objection is at once removed and proved to be incorrect by the results following its administration, in cases not primiparous, where there is 'inertia uteri' with the os fully dilated, the vagina patent, and the perineum not rigid. In just such labours I have been in the habit of prescribing the tartarized antimony instead of ergot, with the most marked results. Unlike ergot of rye it does not produce continuous contractions, but, in the majority of cases, enhances the power and force of the regular pains, and admits an interval of ease. It continues to some extent to exert its contractile influence after the expulsion of the child, and hence reduces the risk of hemorrhage. Of course this result will be modified by the interval that has elapsed between the administration of the antimony and the delivery."* Although we have disagreed from Dr. Parker in what we have said, yet his opinions and observations are well worth following up; for if we can have in tartar emetic a medicine which will contract the upper parts of the womb and dilate the lower, we shall possess a drug with all the advantages and none of the disadvantages of ergot of rye. Moreover, we are sure that our esteemed friend, Dr. Higginbottom, of Nottingham, will rejoice that his views about stopping hemorrhage by emetics, and without stimulants, are gradually making their way into practice. Dr. Parker's remarks respecting the rationale of the action of tartar emetic, are as follows: "The rationale of its action is undoubtedly to be referred to the influence tartarized antimony exerts on the sympathetic system of nerves. The uterus, like the stomach and intestinal canal, is composed of involuntary or unstriated muscular fibre, and is, like the other abdominal organs, largely dependent for nervous supply on the sympathetic system. Now, we all know the action of antimony on these hollow abdominal muscles or canals, that, when administered in sufficient quantity, forcible contractions of the circular and longitudinal fibres composing them, and relaxation of the sphincters, take place; just what I have remarked and recorded as occurring in the uterus. Similarity of texture and nervous supply would then analogically point to that which experience and observation has taught me is in reality the case in reference to the action of this agent in the uterus in the act of parturition. Ergot of rye, while acting primarily and with more energy on uterine muscular fibre, exerts at the same time a secondary or inferior influence on the stomach, often producing, as I have before remarked, troublesome vomiting. Now, the deduction one would naturally draw from the above observations is, that all, or most of what are termed 'specific emetics,' would, through the agency of the sympathetic system, exert their contractile influence on the uterus, and *vice versa*, as just illustrated in the case of ergot,—*i.e.*, therapeutic agents which cause uterine contractions will have a similar action on the stomach and intestinal canal. I have not administered ipecacuanha with the same object in view, but have very little doubt that if tried it will be found to produce like results. Its taste and bulk would, however, other things being equal, give a preference to its more convenient ally, tartar emetic; while, on the other hand, its depressing effects would not be so marked in cases where a continuance of the medicine should be considered advisable. In this connexion I may remark that it is suggestive, and at the same time corroborative of the views here stated, that nausea and actual vomiting, occurring naturally during labour, very generally advance, and often materially shorten the process, by increasing the contractile power of the uterus. I administer the tartarized antimony as follows:—From one to two grains are dissolved in a wineglassful of water, and a dessert-spoonful given every ten or fifteen minutes *ad nauseam;* and if the pains are not increased sufficiently, and there are no contra-indicating circumstances present, it is continued beyond this point. The rectal mode, to which reference has been made, has its advantages, and at the same time, its inconveniences; but there are cases in which it should have the preference."*

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* Edinburgh Medical Journal, Jan. 1863, p. 673.

It seems to be more and more the conviction of accoucheurs that we ought to use the Forceps oftener than we have hitherto done. It not only relieves the woman of useless suffering, but saves the lives of many infants, which would otherwise have been still-born. As soon as the head of the child begins to be compressed in the pelvic bones and the powers of the womb at the same time fail, from that time we may begin to date the danger to the child. Dr. Lawrence compares the practice of the Dublin school, which uses the forceps but seldom, with that of Dr. Hamilton and himself. "Dr. Hamilton tells us of his 90 to 100 forceps cases out of 731 labours, being 1 to every 7 or 8 cases. Drs. Hardy and M'Clintock relate 18 forceps cases out 6634 cases of labour, being 1 to 368·5 cases. In other words, taking Dr. Hamilton's average as 1 to 8, we find he uses the forceps forty-six times for once they are used in the Dublin Hospital! Let us see the results of such contrasted modes of practice. In all Dr. H.'s 90 to 100 forceps cases the children were born alive. In the 18 forceps cases in the Dublin Hospital the mothers perished in 5 and the infants in 8! It will not do to throw the blame of this startling mortality, as is so readily done by the reporters, on the ergot of rye. Others use this drug as a uterine stimulant, without finding it possessed of such deadly potency as it seemed to exert across the channel. We must seek elsewhere for the cause of such disastrous results, and we shall only find it, I fear, in Dr. Murphy's principle having been acted on of 'leaving to nature,' for an unduly protracted period, case after case, which a timely use of the forceps would have conducted to a prosperous issue. My own experience is directly corroborative of what I have here advanced. During the first half of my 1000 cases I used the forceps only eight times, or once in 62·5 cases; during the latter 500 cases I used them twenty times, or once in every twenty-five cases; that is to say, just two and a half times more frequently in the latter than in the former. And what has been the result? The whole of the four fatal cases occurred during the former period! In other words, during a series of 500 cases, at a period when I considered the forceps only called for if the labour was considerably protracted, I used them eight times, and in these eight cases had four dead-born children; while during another, a series of equal magnitude, at a period when I believed that in midwifery, if anywhere,

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* Edinburgh Medical Journal, Jan. 1863, p. 674.

'delays are dangerous,' I used them twenty times, and in these twenty cases all the children were born alive! I leave this fact to teach its own lesson."* It is our duty not only to consider the safety of the mother but also that of the child. Danger to the mother may occur only after a very protracted labour, but the child is in danger long before this period. We ought, therefore, to apply the forceps earlier in these cases, and as practice with these instruments makes the danger but trifling to either mother or child, we would recommend the practice to be adopted more generally. In our own practice in the last nine hundred labours (we have no notes of the previous ones) we have employed the forceps fifteen times. The mother did well in all, and the child was born alive and lived in eleven, and was still-born in three. In one case we have omitted to notice the state of the child, and cannot remember now whether it was alive or not.

Few diseases require more caution as to prognosis than Puerperal Pyæmia. A woman is confined and appears to improve nicely for a few days, rather suddenly the pulse becomes extremely rapid and feeble, but you find no other particular symptom; no pain either on pressure on the abdomen or elsewhere, tongue clean and moist, legs warm, and mind intelligent. How will you diagnose this case? Go back a little in your questions, and you will generally find that a short time before she had a distinct and severe rigor, followed by no reaction; a poison had become imbibed into the system which at once knocked her down, and the quick pulse was only one of its clearest symptoms. As Dr. Latham says, this *would have been* puerperal fever. "The morbid element was taken up and became a quick working poison to the powers of life. The disease had either only just emerged and was cut short, or it never emerged at all. The disease, so to speak, killed itself: it killed itself in the birth. Its first impression was a fatal shock, and then followed not the symptoms proper to its own nature, but the one symptom of coming death, this countless fluttering of the heart." This kind of case illustrates the inutility of morbid anaotmy in some of the most severe diseases. We remember a most interesting series of papers some years ago, involving a most important discussion on the nature of puerperal fever, in which one of our most talented and experienced physicians maintained that cases of puerperal fever were always inflammatory, and required the treatment of inflammation. Another physician maintained (and we think most truly) that puerperal fever was a poison taken into the system which sometimes, as in the case we have just alluded to, struck down the patient at once like as does the poison of a rattlesnake, before any morbid effects have had time to be developed; at other times the constitution throws it off through the liver or digestive canal, or perhaps through the peritoneal surfaces of the abdomen and pelvis, or perhaps through abscesses in the cellular tissue or lungs. This seems to be the correct view to take of such cases, and the lesson which we must draw from the explanation, is never to interfere, if we can avoid it, with the fever and inflammations which nature is setting up to ged rid of a poison. These processes are strictly *conservative.* The fever and inflammation are actually valuable; they show that nature has power to do something to save the patient, and these are the cases which recover. The fatal cases are where these efforts are never made. We are, therefore, sometimes surprised to see these powers and struggles of the system checked and arrested by the practitioner by means of bleeding, leeches, purgatives, and low diet. As a general rule, where there has been a rigor after a labour, followed by a weak and unsteady pulse, we must begin in good time to keep the patient *well up* with *small* quantities of wine or food of a nutritious quality, assisted by the compound spirit of ammonia. It is interesting to see how cordially nature thanks the medical attendant for thus assisting her in her laudable efforts to save her establishment from bankruptcy.

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* Edinburgh Medical Journal, Feb. 1863, p. 718.

It is not difficult to account for the rapid way in which Pus becomes absorbed from the surface of the womb, and carried through the ascending cava to the heart, or perhaps causing coagulation of some portion of the blood which may be carried to the heart or lungs with great rapidity and the most imminent danger. The open veins in the inside of an uncontracted womb are somewhat like the veins in bone. We all know the great danger of phlebitis when the veins of bone are divided and pus is brought into contact with them. These veins adhere to their canals in the bone, and cannot close properly if divided, their mouths are always open and are not readily blocked up by lymph. Hence emboli or pus find a ready entrance. Moreover they absorb rapidly as shown by Cruveilhier's experiment of injecting mercury into the medullary cavity of the tibia of a dog, and which was soon found in its liver. Let us, therefore, more and more exhort the practitioner to *insist* on the womb contracting after labour. No woman is safe with an uncontracted womb, and we should have but few cases of puerperal fever, if the veins of the womb denied all admittance to foreign or self-made poison. We cannot here help quoting the words of Mr. Spencer Wells, on this interesting subject. He says that, "Whether the frequency of plugging of arteries by the detachment of vegetations from the valves of the heart, was or was not overrated, he felt convinced that the converse, or the mechanical plugging of the heart or lungs by fibrinous clots carried from the periphery to the centre, was not yet sufficiently recognised by surgeons as a cause of mortality after injuries, operations, or local disease. If any one still doubted the transmigration of clots, he would remind them that Dr. Druitt had shown in that room a clot where the seat of fracture was most evident. The two broken portions fitted together exactly. He (Mr. Wells) had seen several cases of what some would call pyæmic pneumonia, others pulmonary embolism, in which it was very clear that the origin of the disease had been clotting of blood in the vessels of some part of the body injured or operated on. In one case he believed that an ordinary boil had been the source of the mischief. In another case serious cardiac and pulmonary symptoms, with an almost instantaneous appearance of a rash, like erysipelas or scarlatina all over the body, had followed the application of perchloride of iron to a cauliflower excrescence of the uterus; and in this case clots of various sizes had been found in almost every vessel examined after death."* We think that these remarks fully illustrate what we have said, and show the great need of all accoucheurs having their eyes open with respect to this subject. In the words of Mr. Cooper, of Bury St. Edmunds, who has reported several cases of embolism, "The history of all these cases was undoubtedly one of obstruction to the venous circulation, from the deposition of fibrinous clots; and some of them presented features of interest worth remarking upon. Amongst others, in Case 1 we should notice the similarity of the lung-symptoms to those of pneumonia, and the curious confirmation of the diagnosis by the recurrence of the disease in the opposite side of the body; and also, was the supervention of phlegmasia dolens in any way connected with the accident? In Case 2, the sudden attack of syncope, followed some time after by the occurrence of phlegmasia dolens, left no doubt of the nature of the first attack; and in Case 3 (the patient convalescing from diphtheria), the association of the first attack, from which she recovered, with the condition of the clot found in the left pulmonary artery after death, showing the partial restoration of the circulation, and her consequent temporary recovery. The causes leading to this condition of coagulation in the cases I have related were clearly traceable, primarily, to the blood-condition. No previous history of rheumatism or heart-disease existed in any of them: and further investigation into the exact pathology of this disease will, I have no doubt, corroborate the view taken by Richardson, Walshe, Humphry, and others, that in nearly all these cases (not excepting phlegmasia dolens) the condition of the vessels themselves has no influence on the clotting process, except indirectly through their presenting facilities for the deposition of the clot at the junction of the different large veins, at the projecting angles of the confluent trunks, as well as by their valvular arrangement; and no evidence of any alteration in the internal structure of the vessels from any inflammatory process has ever been found; but that it essentially depends upon a cachæmia or altered blood-crasis, such as obtains in hyperinosis, following acute inflammation, in the cachexia subsequent upon exhausting disease of any kind, and more particularly in the puerperal and diphtheritic crasis. The treatment I have found most available in these cases is that recommended by Dr. Richardson—the exhibition of carbonate of ammonia in large doses, upon the theory that the fibrine of the blood is held in solution by the presence of ammonia, and that its tendency to coagulation is increased by a diminution of the volatile alkali of the blood."*

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* Med. Times and Gazette, Nov. 8, 1862, p. 503.

The subject of Embolism bears on these Puerperal cases in an interesting way. It has of late attracted considerable attention amongst surgeons, and accounts for many of the most sudden deaths after injuries and operations, as well as after labours. A woman after labour, or after some kind of injury of the extremities or pelvic veins, becomes suddenly ill with violent palpitations of the heart, she becomes livid and dies in a few minutes; what is the cause of death in this case? Very probably a clot of blood which is blocking up the pulmonary artery, and which has made its way from the injured vein at a distance. Embolic clots differ from coagula formed just before or after death, in being elastic, brownish, and in the fibrin being "in progress of retrogression." The clots formed at death are soft, œdematous, flattened, and do not so closely plug up the vessels. Nearly all the open mouths of veins which do not close, such as those in the inside of an uncontracted womb, and those in bones, are liable to have these clots carried along their canals. The veins of the womb and pelvis have no valves, and are placed very near the cava ascendens. The woman also is placed in an horizontal position, so that her veins are liable, under favourable circumstances, to carry either coagula or other poisonous matter direct to the heart, and to cause the most fearful mischief. M. Lancereaux describes the condition of the lungs caused by these clots in the following words: "It is evident that an embolus which closes up the trunk of the pulmonary artery, and gives rise to sudden death, cannot cause any important disorder in the pulmonary parenchyma. Supposing such alteration possible, time would be wanting. But it is different when a coagulum comes to be situated in an important division of the artery, closing its

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British Med. Journal, Nov. 8, 1862, p. 489.

canal completely. In this respect, M. Lancereaux expresses his agreement with Virchow, who states that, however complete the obstruction, it produces no alteration in the parenchyma, and, above all, no gangrene of the lungs. At the most, Lancereaux has observed slght diminution of volume, anæmia, or some œdema; and he explains this, physiologically, by the fact, that the pulmonary artery is an organ engaged in hæmatosis, and that the nutrition of the lung is effected, not by this, but by the bronchial arteries. Still (he proceeds to say) pulmonary coagula are sometimes accompanied by a lesion of the parenchyma of the lungs, whether they be the cause of it or not. Pulmonary apoplexy is frequently conjoined with obstruction of the branches of the artery; but it is to be remembered, that this generally occurs in the course of affections of the heart, especially in fatty degeneration; and it is also to be observed, that, under these circumstances, the clot is always situated behind the apoplectic spot, has none of the characters of an embolic clot, and is evidently autochthonic—not the cause, but an effect, of the apoplexy. The same thing may happen in certain cases of tubercular disease, of pneumonia, or even of gangrene. It is, however, important to notice, that there are certain special conditions of the embolic clot which are capable of giving rise to two of the alterations just alluded to—namely, pneumonia and gangrene. These conditions pertain to a special state of alteration of the tissues, in the midst of which the thrombus has been formed: if the coagulation has taken place in the midst of a purulent or gangrenous focus, the coagulum, formed in part of fibrin, and in part of other elements, possesses qualities in virtue of which it may alter the tissues with which it subsequently comes in contact; thus it is that metastatic abscesses often appear in the lungs of individuals with suppurative thrombus of the cerebral sinuses, and in women suffering from metritis or suppurative phlebitis. Thus, too, gangrenous spots in the brain are found in persons who have primarily a gangrene of the lung; and gangrene of the lungs is met with frequently in paralytic individuals, in whom a sphacelus has formed over the region of the sacrum. Particles of fibrin or fragments of tissue, impregnated with pus or septic matters, become the points of origin of secondary foci, purulent or gangrenous as the case may be."*

To show the way and the extent to which Pyæmia may proceed, we notice an experiment of M. Flourens. "He took some drops of pus from the dura mater of a dog and applied them to the perfectly healthy pleura of another dog. The dog died at the end of thirty-six hours, and a double purulent pleurisy was found to exist, the pleura being filled with pus, while no other viscus contained it. Pus was also applied to the abdominal muscles of a perfectly healthy dog. It died in four days, an enormous infiltration of pus having taken place amongst the different muscles of the abdomen. Moreover, pus transported from the dura mater to the pleura of the same animal, caused death on the fifth day, the left pleural cavity being filled with pus."*

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* Medical Times and Gazette, Feb. 14, 1863, p. 168.

There are few more troublesome affections in the female than Fetid Discharges from the Womb. These discharges are frequently too long neglected, the patient does not like to acknowledge them, and the medical attendant often pays too little attention to them. They frequently arise from menstrual discharge retained in the womb from a too small outlet. This discharge, which is often coagulated blood, decomposes and smells offensively. Or the cause may be from imperfect abortion and dead decidual membrane. Many other causes of course exist, but generally we may suspect that they arise from some decomposing structure, which has been retained in the womb, and which this organ cannot easily throw off. The consequences are, a continued state of bad health from uterine irritation, and the repeated recurrence of hemorrhage more or less severe, either at the menstrual or other periods. Whenever a woman applies for the relief of some discharge from the womb, it is our duty to examine to see if we can find any cause for it from cancer or other growths, from abscess, or from the causes just mentioned. We possess in some of our medicines, such as gallic acid, tannin, the turpentines, &c., valuable means of arresting the dribbling of bloody serum, and the secretion of more purulent discharges into the womb, but these medicines will not expel what has already got there. The readiest method of doing this is by *washing-out* the womb; and this is very easily done. The first thing, however, to do is to see that the cervix uteri is wide enough, and if it is not, we must dilate with sponge-tents, or by Dr. Barnes' fluid dilator, or by Dr. Sloane's pessary made of sea tangle, as described in our last volume. There are many other methods of accomplishing this dilatation, but we should give the preference to Dr. Barnes' instrument. The canal having been dilated, the next thing to be done is to wash out the womb "by injecting freely a stream of warm water through a double catheter, such as is used in injecting the bladder." Dr. Matthews Duncan has written a very interesting paper on this subject which will be found in the body of this volume. As a very simple and extemporaneous method of washing out the womb, and particularly if you wish to apply a solution of tannin, or a weak solution of nitrate of silver, or any other styptic, we would recommend our own simple method. Introduce a gum elastic male catheter without the stilette into the womb, fill one of the small balloon-shaped india-rubber bottles (such as are sold at any of the india-rubber shops, with a small tube at the outlet) with the solution which you mean to inject, introduce the nozzle of the india-rubber bottle into the catheter, and now press the bottle and the contents pass at once into the womb. This can be repeated as often as you like. The instrument is very cheap, very handy, and one of its great advantages is that you can have one for each patient, and can leave them at the house of each case till you pay your next visit.

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* Medical Times and Gazette, April 4, 1863, p. 359.

It seems that *one-half* of the cases of Vesico-Vaginal Fistula follow the use of instruments. Out of 58 cases reported by Mr. Baker Brown, 47 were over twenty-four hours in labour, and 39 were as much as thirty-six hours or more; 7 were two days, 16 were three days, 3 were four days, 2 were five days, 2 were six days, and one was seven days. Instruments, however, could not fairly be charged with the blame of these cases, as it will be seen that in most of the cases there was more or less *impaction*. On the other hand, it seems that if instruments had been applied early in the cases, the fistulæ might have been prevented. In almost all cases of impaction it is recommended by some to have recourse to instruments early.* Let us not forget these important statistics, as they will often quite exonerate the use of instruments in cases for which the accoucheur may be blamed by the public. In these cases of fistulæ, the American operators, Drs. Sims and Bozeman, denude the surfaces with scissors. Mr. Brown prefers the knife. He first marks round the orifice with the knife as much as he means to take off, and then by means of a pair of fine forceps makes the edges tense, the edges can then at once be denuded and much time saved. He uses the silver-wire ligatures, and allows them to remain in never less than nine days; they may remain in for a much longer period, and seldom or never produce ulceration, as does the silk ligature. It is curious to see the difference of opinion on this question of ligatures between two such eminent men as Mr. Baker Brown and Mr. Spencer Wells. Whilst Mr. Brown's practice is as just stated, Mr. Wells prefers the silk ligature, and pays a just tribute of praise to the American surgeons, who have advanced so rapidly of late, that if we do not take care they will excel us in many of the most important operations. We express this opinion to induce the British surgeons to look about them, as we can assure them that in our reading of the American journals we are struck with the advance of our Transatlantic cousins in the science and art of medicine. To express Mr. Wells' views, we had better quote his own words. He says that he "congratulated Dr. Aveling upon the successful result of his interesting case, and heartily concurred in the tribute he had paid to the services of Dr. Marion Sims. But he (Mr. Wells) had begun to doubt whether the success which had followed the operation for the cure of vesico-vaginal fistula of late years was so much due to the use of wire sutures as to the improvements which Dr. Sims had originated in the mode of bringing the fistula into view, accurately paring the edges, and bringing them into perfect apposition. Provided the edges of a fistula were thoroughly pared, and kept in close apposition, it was probably of little importance how this was done. A year ago he (Mr. Wells) was as strongly in favour of metallic sutures as anybody; but latterly a wider experience had taught him that it is only after five or six days that wires show any advantage over silk, and before that time the sutures ought to be removed. Then silk offers the great advantages over wire of being more easily applied, of not requiring so large a needle to pass it, of the ends being much less irritating, and of being more easily removed. After many comparative trials on different parts of the same wound with wires of silver, iron, lead, platinum, and aluminium, and with fine catgut, horsehair, telegraph wire, india-rubber thread, and the fine strong silk known as 'Chinese twist,' he had become convinced that wires offered no advantage over silk, while silk offered many advantages over every other material used for sutures. In a recent case he had closed a vesico-vaginal fistula by five silk sutures, and perfect union resulted, although no catheter was used. The supposed necessity for the use of the catheter after closing vaginal fistulæ, was another error which time was correcting. The urine is by no means so irritating a fluid as some believe. The lower orders use it as a lotion to the eyes and to sore legs; and it certainly cannot differ much from the dilute saline solutions constantly prescribed as astringents or stimulants. The use of the catheter is the most troublesome part of the after-treatment, and often most distressing to the patient. One of his patients really could not bear it, yet she did perfectly well; and lately he had not used it at all, union taking place quite as well as when it was used, and the patient being much more comfortable."*

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* We must carefully mark the distinction between *protracted* labours and cases of *impaction*. A labour may be protracted for a day or two without there being any degree of pressure of the parts against the pubes, but when the head is fairly *impacted* and is pressing the parts without intermission against the bones of the pelvis, the almost necessary consequence will be devitalization of the particular part pressed upon, followed by a slough. It is in these cases of protracted labour combined with impaction that the forceps, if possible, ought to be applied earlier than is generally done.

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* Lancet, Jan 31, 1863, p. 119.

Sometimes, although very rarely, you see a Vesico-Uterine Fistula. You find that urine escapes down the vagina, but on examination you cannot find any communication through the parietes of the vagina. If, however, you dilate the os and cervix uteri, you will probably find that the fistula is in the cervix; such was the case in a patient of Mr. Lane's. "On examination with the speculum, there appeared to be nothing the matter with the vagina or urethra; the vesico-vaginal septum was perfectly sound throughout; but when the os uteri came into view, a small quantity of fluid was observed to flow from it into the cavity of the speculum. By digital examination, the os uteri was found to be somewhat larger than natural; it would just admit the tip of the finger, with which, about half an inch above the os, and in the canal of the cervix uteri, an opening could be felt on the anterior surface, leading towards the bladder. A sound passed into the bladder by the urethra could, with a little manipulation, be brought into contact with the finger in the cervix uteri. The opening appeared to be rounded, and to be about the size of a pea, as if a small piece of the uterus had been punched out." What are you to do in such a case as this? Will you adopt M. Jobert's operation, which "consists in making free lateral incisions through the os uteri and through the upper end of the vagina, so as to convert the lower part of the cervix uteri into two flaps, anterior and posterior, by separating which, the fistulous aperture might be brought into view, so as to admit of its edges being pared and sutures being applied. He suggests that these incisions should pass completely through the lower end of the uterus, and be continued into the upper end of the vagina, until they penetrate into the loose areolar tissue of the broad ligament. These incisions, he says, render it easy to get at the aperture of communication, and enable the surgeon to perform his manipulations with facility, and without any obstacle whatever." We think this practice would be difficult, and should decidedly recommend Mr. Lane's method which he describes as follows: "The idea which suggested itself to me to reach the fistula was to enlarge the opening, in the downward direction, by dividing that portion of the uterus which was situated below the fistula, and by continuing this incision into the upper end of the vagina in the longitudinal direction. I should thus have obtained a large longitudinal opening, the upper angle of which would have corresponded to, and have been formed by, the fistula itself. I believe that this would have enabled me to get at its upper end, where the fistula was placed, and which was the only difficult part to deal with, so as to apply the necessary sutures. If, however, I did not find sufficient room, I could have obtained more by making lateral incisions in the cervix uteri, in the direction indicated by Jobert, but of less extent. This plan would, doubtless, have been attended with considerable difficulty, still, I believe, it was not impracticable—the danger would not have been great; and, if I could have succeeded in closing the uterine portion of the wound in a first operation, there would have been comparatively little difficulty in dealing with the vaginal portion on a subsequent occasion. But there was the alternative of the second operation, suggested by Jobert in his treatise, but which I did not then know that he had ever put in practice. It consisted in closing the os uteri by suture, thus shutting off the communication between the uterus and the vagina, but leaving the fistula itself uninterfered with. When this was done, the urine would still, of course, get into the uterus, but it could not pass further; and the remedy, so far as the escape of urine externally was concerned, would be practically as perfect as if the fistula itself had been closed. Future pregnancy would, of course, be impossible, and menstruation must be performed through the bladder, by means of the fistulous opening; but these disabilities appeared, to my mind, to be more than compensated for (especially taking into consideration that the patient was forty-five years of age) by the avoidance of all risk from the operation itself, by the greater certainty of success, and also by the fact, that should it not succeed, or should any difficulty in the matter of menstruation be subsequently met with, a slight incision through the new line of union would at once restore the parts to the same condition as before the operation. The patient being placed in the lithotomy position, Bozeman's speculum was introduced, and held so as to depress the posterior wall of the vagina, and bring the anterior wall and the os uteri into view. The anterior wall of the vagina, just below the os uteri, was then seized with one vulsellum, and the posterior lip of the os uteri with another. By these means, with a little traction, the parts were drawn down within convenient reach, and were held in the required position by assistants. I then, with a narrow knife, freely denuded the edges of the os uteri, and brought the raw surfaces together with four silver-wire sutures, passed by means of the tubular needle. I brought the ends of the wires through the holes in a thin leaden plate, and fastened them with perforated shot, after Bozeman's method. I greatly prefer, in this and all similar cases, the use of the leaden plate to the fastening the wires by simply twisting their ends. The slight pressure which the plate exercises is of great service in keeping the part steady and motionless; and it also acts most advantageously in keeping the sutures throughout at a proper distance from each other, thus preventing that gaping apart of the wound in the intervals between the sutures which is so likely to take place if they become unduly approxiduce it, in withdrawing it we cut but at one side; we then, should it have a regulating screw, double the projecting power of the blade, reintroduce it, and use the wound just made as a support for the sheath whilst we incise the opposite side. The unfixed state of the uterus and the density of the parts would necessitate all these proceedings if the surgeon wished to have both sides incised equally. Suppose we use a probe-pointed bistoury, it would require the exercise of both skill and care to use it safely for our patient, and would necessitate the use of the speculum. These are the reasons why I prefer the probe-pointed knife to any other instrument. The knife I at present use is not so full where it springs from the probe-point as that which I described in my former paper, and has a rather deep circular groove at one inch and three quarters from the point, for the purpose of serving as a guide to the finger, or to the eye when the speculum is used, as to the depth to which the knife has penetrated. By having it marked so far from the point, we can easily judge of the proper distance to introduce it, both in the healthy state of the organ and in those cases in which we find it hypertrophied." "In those cases of dysmenorrhœa and sterility in which we have reason to suppose that obstruction exists, but where we find the os and cervix dilatable, I think that our proper proceeding is to introduce the leaden tube at once by means of the dilator without incising the parts. I have yet had little experience of this proceeding, but think it will be found valuable. In the dysmenorrhœal case which I have before mentioned, whether it was that the patency of the canal was preserved for the time, or the nervous irritability of the organ was diminished by the presence of the foreign body, the comparative freedom from pain was quite remarkable. Before using the knife, the surgeon must not forget to pass a No. 1 bougie. He should also see that the knife is in good order, and that the cutting edges proceed *directly* from the narrow neck of the probe-point. The leaden tube should be well turned out at the outer end, so as to prevent the possibility of its passing within the incision."

We think that women are almost as liable to affections of the rectum as of the womb. Hæmorrhoidal Tumours, in women, sometimes arrive at a very great size, prolapsing at every stool, and even in the act of walking, bleeding copiously, and gradually undermining the health. It becomes necessary at last to relieve the patient. There are several ways of doing this. In many cases you will succeed by applying nitric acid to the red prolapsed surfaces, just after a stool has protruded the parts. In other cases you will have to introduce Weiss's three-pronged dilator, and apply the acid higher up. In still more severe cases you will have to ligature the mass of the tumour, and after pushing the parts up, will have to leave them to slough away. Mr. Henry Smith recommends a modification of these plans when the patient is unwilling to submit to the usual methods, and in some cases he recommends his new plan even in preference to the others. We cannot do better than give his own words. "He had found so many patients object to the use of the ligature from a dread of dangerous consequences, that it was most desirable to substitute some method which would be equally successful as regards curing these diseases, and yet be free from the danger attendant upon the ligature. He had made use of nitric acid alone, and had been very much pleased with this agent; but he found that severe cases would not be thoroughly cured by it, and that in many, where such great benefit had accrued as to constitute a cure, sooner or later symptoms of the disorder would recur. He had therefore resorted of late to the method originally practiced by the Dublin surgeons, and adopted by Mr. Henry Lee—namely, that consisting of a combination of excising and cauterizing. He (Mr. Smith) had, however, in the course of his practice, found the clamps ordinarily used to be somewhat awkward and inefficient, and he had had one recently constructed for him by Mr. Matthews, the blades of which were so shaped that they could readily be applied to the anal orifice, and would meet together closely by means of a groove. Moreover, through the handles of the instrument, which were made very thick and strong, was inserted a light but powerful screw, by means of which the compression could not only be put on the part to be removed, but could be as gradually taken off, and the instrument could be fixed at any period of the operation. This was a great desideratum in the process, for by this mechanism the part could be gradually and thoroughly strangulated, and the chance of bleeding could be entirely obviated. The author showed the instrument, and minutely described the proceeding which he adopted, and which consisted in first seizing the part to be removed by means of a vulsellum, then engaging the tumour in the blades of the clamps, which were next thoroughly brought together by means of the screw, and then fixed. After a few moments' delay the free portion of the tumour or prolapsus was cut off by sharp scissors. The cut surface was then carefully wiped, and strong nitric acid or actual cautery applied. The blades of the clamp were then gradually disengaged by means of turning the screw, and in this way any bleeding vessel was readily noticed and secured, and further cauterization could be made or a ligature placed on it if needful. The advantages of this plan of treatment consisted in its being free from danger if precaution be taken perfect immunity from inflammatory process with which the silver-wire had perforated the peritoneum, even in three places. In a case which I published last year in the Lancet, I say: 'A patient, aged seventeen years, was admitted into the Birmingham Workhouse Infirmary on the 18th of December, 1858. He was the subject of varicocele on the left side, which had existed for three years, and to so great an extent as to prevent him from following his employment. Being desirous of radically curing him, I adopted the method of M. Ricord, which I had seen him perform with success; but instead of using the silken ligature which he used, I tried to do the same thing with silver-wire—namely, between the vas deferens and the veins, I passed by means of a needle, a double wire, and by the same apertures in the opposite direction, anterior to all the veins, another wire. By engaging one free end through the loop of the other one on both sides, the veins were, by traction on the wire, compressed. This traction was kept up continuously by means of an elastic watch spring, bent in the shape of a horse-shoe. In ten days' time, thinking the varicocele cured, and wishing to remove the wire, I tried to do so, and found I could not, though it was easy enough with silken ligatures. I therefore cut off the wires as short as I could, trusting to the innocuity of silver wires to cause no harm. In another week the punctures were well healed, and the patient was discharged, the varicocele then appearing perfectly cured. Seeing him again in about a year's time, I found upon examination, that all things remained as when he left, and he stated that he had resumed his work immediately upon leaving. He himself was totally unaware of there being any wires in his scrotum.'"*

In Ovariotomy, we think Mr. Hutchinson's suggestion a good one, namely, that when the cyst has been partially emptied, the woman should be turned to the side opposite to that from which the tumour grows. Make an incision just large enough to admit the hand, which may be introduced carefully to break up adhesions; next puncture the cyst by a large trocar, and when nearly empty turn the woman on her side, and when the right time comes secure the peduncle by the clamp and cut away the tumour about three inches above it. Secure the abdominal incision by hare-lip pins. It would seem that by this simple procedure there is less chance of the fluid or blood escaping into the peritoneal cavity, and it is much easier to get the tumour out as its own weight assists materially. In cases where there are several cysts, the side position is still more desirable, as these can be more easily reached and punctured in succession, without the fear of fluid escaping into the abdominal cavity.

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* Lancet, February 28, 1863, p. 233.

Dr. R. Lee strongly opposes the operation of Ovariotomy, and throws doubt on the truthfulness of the statistics of the operation. He says that it is his opinion that many fatal cases have not been reported, indeed he seems to know of many fatal cases which have not been really reported. Although it is our own opinion, from what we have read, that in properly selected cases, and when operated upon by those who make this branch of surgery a favourite study, the operation is more successful than Dr. Lee's opinion would lead us to conclude. Yet we cannot but regard the opinions of so eminent a man as Dr. Lee as deserving of our most serious attention before operating in these cases. He says "It is very notorious that numerous fatal cases of ovariotomy had occurred of which no report had ever been published, and all attempts to remove the veil which concealed them had been fruitless. He was told that the statistics of ovariotomy were to form the groundwork of all decisions respecting ovariotomy, and that they were worthy of trust. An attempt had been made to institute a comparison between the results of ovariotomy and great surgical operations. A chronic ovarian disease, after lasting for years, and not threatening life, bore no resemblance to a case of strangulated hernia, an aneurism ready to burst, or a shattered limb with hemorrhage from the large arteries. Ovariotomy could not be compared to any of the great operations of surgery. In the cases of ovariotomy called unsuccessful, the patients had been suddenly deprived of life by violence without any necessity. But it was not from some occasional cases of recovery from the operation, such as those related that evening, that a correct opinion of ovariotomy could be formed. All the facts must be taken into account, otherwise no sound judgment could be formed of the difficulty of the diagnosis and the danger of the operation. It appeared from his analysis of 162 cases, that a successful instance occurred in America, and was published in the Edinburgh Medical and Surgical Journal in 1822. He had thought that this was the first case; but it was now stated that the operation had been performed by Dr. M'Dougal before. This was a matter of no importance. It could not be denied that the publication of a successful case took place in 1822; and he had supposed Mr. Lizars, in consequence of this case, had recourse to operation. In Mr. Lizars' first case there was no ovarian disease to be removed. In the second, the disease was removed, but the other ovary was diseased and could not be removed. This patient was afterwards seen by some surgeons in London. He believed Mr. Lawrence saw her, with a great cicatrix of the abdomen, and a large mass of disease within. The third patient operated upon by Mr. Lizars died. In the fourth case there was no ovarian disease to be removed.

About 1829, a case similar to this was seen in Guy's Hospital by Dr. Gooch. In 1826, Dr. Granville made an incision of nine inches long through the abdominal parietes, and a large ovarian tumour was brought into view, which had such extensive adhesions that it could not be removed. In 1829, Dr. Granville operated again, and removed a tumour weighing 9 ℔s., which was supposed to be ovarian, but it was a large fibrous tumour of the uterus. Dr. Lee examined this tumour in the recent state, and ascertained that it had adhered to the fundus uteri by a thick peduncle, around which a ligature had been applied and the root cut across. A portion of small intestine which had come in contact with the incised root became inflamed and gangrenous, and the patient soon perished miserably. The preparation of the parts was long in the possession of Mr. North. Dr. Granville was about to operate upon another patient, but Mr. C. Clarke recommended the patient not to submit to the operation, and she lived some years after, and died a natural death at Barnes. Dr. Scott examined the body, and presented the tumour to Dr. Lee, which was the uterus with a fibrous tumour embedded in its walls of the size of three human heads; it was in Dr. Lee's collection at St. George's Hospital. Dr. Granville had far greater experience in the practice of midwifery than any who had that evening spoken. In 1835 Mr. Jeaffreson performed the operation and the patient recovered. From that time to the present the operation had occasionally been performed with success, but as often, he (Dr. Lee) believed, with fatal results, if the whole truth were revealed. About the same time Mr. King attempted to perform the operation, but the ovarian disease could not be removed, and the patient died. In 1834 he repeated the operation with success, and again in 1836. Then followed three successful cases and one fatal. Then followed six fatal cases running, in which the operation was performed by Messrs. Hargraves, B. Phillips, Aston Key, B. Cooper, and Mr. Greenhow. In looking over one column of his 'Analysis,' he (Dr. Lee) could see 'Died—died—died—died—died—died' in a few days—he might say killed. All these operations were performed by distinguished surgeons, and none of them repeated the operation. In fact, the operation required undoubtedly great hardihood. So far it was an operation such as had been described. He would refer those who wished to know all the facts to the remainder of the 'Analysis,' by which it appeared that of 162 authentic cases in which the operation had been undertaken, in 60 the ovarian disease could not be removed, 19 of which proved fatal; of the remaining 102 cases in which the operation was completed, 42 terminated fatally. From these facts he concluded that ovariotomy and the Cæsarean section were the two most dangerous operations which could be performed on the human body."* On these important statements of Dr. Lee we prefer publishing the comments of the Editor of the Lancet, which we think very valuable. "Practitioners have long been divided with respect to the value of ovariotomy as a surgical proceeding. Is any further light thrown upon this subject by the discussion in question? We can scarcely think that there is. The tendency, on the one side, was to overestimate its value; on the other, unjustly to depreciate it. What is really wanted is a more clear description of those cases in which the operation may be safely practised. That we are deficient at the present time with regard to the diagnosis of these cases the impartial observer will admit. The way to improve our knowledge on this point is not by wholesale assertions as to the facility with which we can determine which are the proper cases for operation, nor, on the other, can a general condemnation of the proceeding, and an exaggeration of the difficulties of forming a correct opinion, have any other effect than to involve the question in still further obscurity. It cannot be admitted at the present moment that our acquaintance with the nature of ovarian disease is so complete as the advocates for ovariotomy assert, or that we are so ignorant respecting it as the opponents of that operation so unhesitating contend. We think it is to be regretted, as the Council of the Society determined that the 'facts' which Dr. Lee had forwarded should not be read, that the other portion of his paper should have formed the text of the discussion. Without these facts his communication amounted to mere assertions and objections to the operation, and gave his opponents a vantage ground which was scarcely fair to the veteran obstetrician. The Council, no doubt, had good reasons for withholding the letters which Dr. Lee had made the appendix to his short paper, and which, like the postscript to a lady's epistle, formed in reality the more important part of his communication. One result however, of the recent discussion will not be without benefit. It will show that whatever has hitherto been the success of ovariotomy in the hands of certain operators, the diagnosis of ovarian disease is by no means without its difficulties, nor the 'simple' operation without its complications and dangers. The truth, as is usually the case in such questions, lies probably between the two extremes. Under favourable circumstances, and in well-defined cases, ovariotomy is a valuable operation. That it may sustain its reputation, it is essential that its advocates should be less sanguine in expressing their opinions. It is a duty not less incumbent upon the opponents of the operation to examine with candour the evidence in its favour."†

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* Lancet, Nov. 22, 1862, p. 569.

† Ibid, p. 572.

PRACTICAL MEDICINE.

DISEASES AFFECTING THE SYSTEM GENERALLY.

ART. 1.—ON SCARLET FEVER COMPLICATED WITH RHEUMATIC AND CARDIAC AFFECTIONS.

[It seems now pretty well decided that cases of scarlet fever occur, complicated with rheumatic affections of the joints, and even with well marked pericardiac and endocardiac inflammation. Dr. Richardson in his Clinical Essays, article "Scarlet Fever," p. 85, says:]

That he once attended four children in one family for scarlet fever and that in two of the cases well-marked symptoms of rheumatic fever set in on the second day of the eruption. In one, the endocardial membrane became affected. He writes—"I could not make out satisfactorily any proof of hereditary taint, as accounting for the rheumatic complication; but there it was, and there was the fact, in spite of any hypothesis to the contrary, that two diseases may exist in the same body at the same time."

[The connexion of affections of the pericardium and endocardium with scarlet fever has been long recognised, but does not seem to be considered as a frequent cause of disease of the heart. It is not mentioned by such standard authors as Walshe and Markham. Dr. West, in his work on "Diseases of Children," writes:]

"In two cases of pericarditis, in three of acute and one of chronic endocarditis, or in six out of thirty-nine instances, the disease of the heart was traced to an attack of scarlet fever. The cardiac symptoms did not manifest themselves in the acute stage of the affection, but during the progress of desquamation."

In reference to the connexion of the heart affection in scarlet fever when complicated with rheumatism, the following quotation from Dr. Watson will be interesting:—"I have several times, when the rash of scarlet fever was disappearing, known pain and swelling of the larger joints to supervene, simulating closely the local phenomena of sub-acute rheumatism; and I have noticed that the painful joints were eased and benefited by friction—a circumstance which may help to distinguish this articular affection from true rheumatism. Another distinctive circumstance seemed to be that, although all these patients were children, the heart in no instance became implicated in connexion with the tumid joints. Upon this point, however, my own experience may have been fallacious. Dr. Scott Alison has recently invited attention to the subject, in an interesting essay on 'Pericarditis a Complication and Sequela of Scarlatina.' Accepting his facts, I should ascribe the articular affection and the cardiac affection, whether they occurred together or separately, to one and the same cause, viz., to the retention in the blood of a poisonous excrement, by the default of the principal emunctories, and especially the kidney."

[In one case which occurred to Dr. Budd, at King's College Hospital, the report of the autopsy relates that there was "some recent lymph on the surface of the heart, and a few ounces of serous fluid in the pericardium." In this case no abnormal sound was heard before death, though the action of the heart was irregular and feeble. We do not consider this a common complication of scarlet fever, yet that it does occur we ourselves can testify.]—*Med. Times and Gazette, Nov.* 29, 1862, *p.* 572.

2.—A CASE IN WHICH A DISEASE, LIKE MEASLES, AROSE FROM AN UNUSUAL CAUSE; WITH REMARKS.

By Dr. Henry Kennedy, A.B., one of the Physicians in ordinary to Sir P. Dun's Hospital.

A young gentleman of fifteen years of age, rather under-sized, but of a high order of intelligence, returned to school after the summer holidays, being then in perfect health, as he entered the school-room one of his play-mates met him, holding a paper bag, with some kind of powder in it, in his hand, and before he was aware, had dashed a handful of the powder in his face; and there can be no doubt that some of it got not only into his eyes, but down his throat—for he was laughing at the moment. The powder turned out to be flaxseed-meal, which, by some accident, the other boy had found in the room.

The result was truly remarkable; the boy was at once seized with smarting and watering of the eyes, running from the nose, cough, and dyspnœa. With some difficulty he made his way home—a distance of an English mile. By the time he reached it his face had become much swollen, the eyelids and eyes very red, and the dyspnœa urgent. The excitement, too, of the system generally was very great; and all this within two hours of the accident. When seen the following day he had, except the rash, all the look of a boy suffering from a sharp attack of measles. His face was still swollen, his eyes were injected, and had a strange dark-red line round them, giving a very peculiar expression to the countenance; and he had a constant loud cough, with dyspnœa. His pulse was 120. Two years previously I had attended him in a well-marked attack of measles, with cough.

On hearing the history of the case, I confess I thought that quiet and a little time would suffice to get him well; and so he was only directed to inhale the steam of boiling water; and, as his distress was referred mainly to the larynx, a small mustard poultice was directed to be applied over that organ night and morning. In this expectation, however, I was much disappointed; and finally, after waiting a few days, when a considerable amount of general bronchitis had supervened, I was compelled to treat the case as if it were ordinary measles, by salines, including tartar emetic, and blisters; and by the end of three weeks, and not till then, could he be pronounced well. The last symptom which remained was dyspnœa; for this he got small doses of the oxide of zinc with markedly good effect. The boy is now perfectly well.

This case I set down at the time of its occurrence as anomalous—for I could scarcely believe that such symptoms would follow such an accident; but, on the other hand, the accurate history given me, and the direct connexion between the accident and the immediate supervention of the symptoms, could barely leave a doubt that the two stood to each other as cause and effect. And so matters remained, and the case had got quite well, when the paper referred to came under my notice. This paper will be found in the American Medical Journal for July, 1862, its title being—"Remarks on Fungi, with an Account of Experiments, showing the Influence of the Fungi of Wheat Straw on the Human System; and some Observations which point to them as the Probable Source of 'Camp Measles,' and perhaps of Measles generally. By J. H. Salisbury, M.D., of Newark, Ohio." Some of the cases given in this paper had been transferred to the Dublin Medical Press, and there caught my eye. Its general tenor is to show that soldiers are very liable to a form of measles, when obliged to sleep on straw which is old or musty; and two striking examples are given where large numbers of men were so attacked, measles not being in the camp at the time. The author then goes on to state that he treated straw in such a way that it became mouldy within 48 hours; in other words, fungi, of which accurate drawings are given, were produced. With these fungi he then inoculated 13 persons, the result being that, within two days, a disease exactly like measles in some, including the rash, followed.

These experiments appear to me to prove conclusively that the fungi of musty straw are capable of generating a disease which it seems next to impossible to distinguish from measles. It may be well to give one or two of the cases, condensed for the sake of brevity:

"*Case.*—At 10 o'clock, p.m., February 11th, 1862, I inoculated my arm with the spores of the fungi of wheat straw. The straw was the same kind as that used for beds at the camp.

"Wednesday 12th. Perfectly well; no inflammation at the point of inoculation.

"13th. Very slight redness and itching at inoculated point.

"14th. Got up with a feeling of lassitude and nausea; redness and itching on the increase; had difficulty in keeping warm; occasional sneezing; eyes sensitive; a peculiar feeling about the scalp.

"Saturday 15th. Lassitude, sneezing, &c., continue, with flashes of heat over the whole body; inflammation of wound on the increase; the peculiar burning congested feeling over the scalp has also increased, with pains through the forehead and temples; a few blotches have made their appearance on the face and neck; eyes weak and inflamed; a heavy oppressive feeling about the chest; and the throat dry and irritated, as if I had a severe cold."

It is enough to say, that all the symptoms detailed continued with considerable intensity till Wednesday, 19th February, when the writer found himself much better. He then again inoculated himself, but with no result whatever. The next case he gives is that of his own wife, who, on the second day after the inoculation, began to suffer from severe cold, causing hoarseness and great constriction of the throat, with very considerable fever, sneezing, and sore eyes; also a slight rash visible. These symptoms lasted a week, and subsided gradually; the attack having passed downwards, as it were, to the stomach and bowels.

There are also cases given in the paper where this artificial inoculation, as it may well be called, seemed to act as a prophylactic, by saving children from the ordinary measles, which disease was in the house at the time. But this point is only alluded to here to show the extent to which the author has carried his investigations. It is now very many years since Darwin, if I recollect right, proposed that the tears of those affected with measles should be used for a similar purpose.

To the facts detailed in this paper the case which came under my own notice is now to be added; and no matter in what aspect it is viewed, it seems to me to open up a question of the greatest interest and extent. For if there be other agents than the fungi of wheat straw capable of generating a disease like measles, where will such powers end. All vegetable and animal substances, too, while in a state of decay, are known to become mouldy—producing fungi or spores; and to such an extent does this go, that even living beings, including ourselves, when in bad health, almost constantly produce them. The revelations of the microscope are, in this point of view, truly startling; showing us that diphtheria, and some of the most inveterate diseases of the scalp have the closest connexion with cryptogamic growths. The potato and the vine-blights, too, come exactly within the same category; many other examples might be enumerated; and I would refer any one particularly interested in the subject to a paper published in *M'Millan's Magazine*, for October, 1862. True it is, that in all these instances the fungi are the result, not the cause, of disease. But what I now wish to call attention to is the great universality of these bodies, whether of animal or vegetable origin, coupled to the fact, which may be considered as absolutely established, that some of them, at least, are capable, when applied to the human frame, of generating disease. And if we farther reflect on the great facility with which these bodies may gain access to our frames, either in our food or the air we breathe, we have a source of disease opened up of which the importance can scarcely be over-estimated. For a long period the opinion has prevailed that epidemics are caused by some such bodies as these. But proof was wanted in support of this idea; and here now we have gained the first step. For it must be repeated again, the experiments of Dr. Salisbury are conclusive:—That certain bodies which are being constantly generated in vegetable matter, are capable of causing certain diseases when inoculated into the human frame. Nay, further, that inoculation is not essentially necessary; and on this point the case detailed by myself seems to be most important, for it shows that the mere contact of fungi with the mucous membranes is sufficient to cause disease, and of a very severe character.* The case also proves that other fungi than those of wheaten straw are capable of inducing an affection very like measles. This last point opens out a field of the very widest for investigation. Thus measles presents itself to our notice under different forms. We have this disease with and without cough ; a distinction long recognised, and, I believe, correctly. Again, at some periods the throat is very much engaged, and at others quite free ; and I myself have seen measles rendered most serious by its complication with acute dysentery. May not such differences, or more correctly varieties, arise from different fungi ? One kind prevailing at one period, and a different kind at another. Or may not the way by which the fungi get into the system cause a difference in the results ? Thus, in the case I have just detailed, the eyes, mouth, fauces, and windpipe were naturally the parts which suffered—the irritation thence spreading to the lungs. But if, in place of this mode of ingress, some of the meal had been at once taken into the stomach, what might not then have been the consequence ? Would it have been dysentery or other intestinal irritation ? Though, with our present knowledge, these questions cannot be answered, they yet seem to me all-important ; and, no doubt of it, the time will come when they will be cleared up, and so a great step gained in practical medicine.

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* Some may think that the case given by myself may be explained in a different way; that the symptoms may have been caused by the mechanical effects of the meal, and arose too soon to attribute them to the effects of fungi. This view of the case may, of course, be held; but the following reasons induce me to keep to that stated in the text:—It was clearly established that the meal thrown in the patient's face was mouldy; and since then, my friend Dr. Kidd, the present editor of this Journal, has succeeded in bringing meal of the same kind into a similar state, and, by means of the microscope, detecting in it fungi very like, if not identical with, some of those figured in the plate of Dr. Salisbury. These fungi Dr. Kidd showed myself; so that the fact is absolutely established, that fungi may have existed in the meal used by the

Other considerations arise out of the foregoing statements. Thus they afford a reasonable solution of the cause of certain diseases—as, for instance, the hay-fever or asthma ; and may not the affection known as influenza come into the same category ? The strange effects, too, of certain substances in the form of powder, of which ipecacuanha is a good example, may result from the same agency ; and when smells or odours are of such a kind as to create fainting, nausea, or even vomiting, are there not good grounds now for supposing these may be due to certain particles floating about in the air, and necessarily taken into our frames ?

Nor, in a practical point of view, are the preceding facts of less moment ; and the following deductions may, I think, be fairly drawn from them :—

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boy in the school. But then, it will be said, the effects of the fungi arose too soon, for the symptoms were immediate. This I think may be questioned. There is certainly no comparison whatever between the effects of inoculation on the skin and on a mucous surface. If a poison, like putrid pus, be applied to the eye, will it not begin to act at once ? and so of the mouth, nose, and stomach. I have seen a sore throat begin at once, from inspecting the same in another. Are not all the stronger poisons, such as corrosive sublimate, arsenic, &c., instantaneous in their action, provided the dose be large and the stomach empty. Nor is it to be forgotten, that in the case detailed, the dose, if I may so speak of it, was large, and applied to the eyes, nose, and mouth at the same moment. For these reasons, then, I consider this case an example of a disease, like measles, being caused by the application of the fungi which are produced in flaxseed-meal which had become mouldy.

1. That certain acute diseases affecting the throat and air passages may be caused either by inoculation of certain vegetable fungi or by direct contact of the same with the mucous membranes.

2. That, as far as is yet known, the diseases so produced seem to have the closest resemblance to measles.

3. That the vegetable fungi which have long been admitted to exist in certain chronic diseases, as those of the skin, show an impaired state of the constitution; and hence the importance of combining a constitutional with a local treatment in their management.

4. That when vegetable fungi cause disease by coming in contact with the mucous membranes of the head and chest, we have now fixed data for the administration of emetics; which, by their direct effects, may thus cut short disease in its early stage.

P.S.—A second paper, by Dr. Salisbury, has, I find, been published in the American Journal for October, 1862. It is also a very valuable communication, as showing, and in a very clear way, the prophylactic power which the inoculation of straw fungi exercises over the human frame, even when exposed to the contagion of ordinary measles. But as this part of the subject does not come within the scope of my present remarks, I can only refer my readers to the paper itself for the details, which are of high interest.—*Dublin Quarterly Journal, Feb.* 1863, *p.* 61.

3.—ON SARRACENIA PURPUREA.

By Dr. Frederick W. Morris, Halifax, Nova Scotia.

Claiming, as I have a right to do, I will not say the honour, but the priority, of announcement to the profession of the Sarracenia purpurea, as the great anti-variolous remedy and elixir for suffering humanity,—being the first medical man in history to employ it in this practice, and in the face of a population of thirty thousand in this city of Halifax, when not a whisper of it was known to other medical men in the community, military, naval, or civil,—it is with no little assurance that I feel myself authorized to come forward upon the present occasion, and assert that the use of Sarracenia purpurea for the cure of small-pox, and I may add of the entire family of the exanthemata in the Cullenian sense, and arising from specific contagious poison, is entirely due to my agency, in the month of April, 1861, several months antecedent to any notice of it by any other medical man. The professional administration of this marvellous vegetable for variola dates from the Halifax Visiting Dispensary, Nova Scotia, and from my hands alone, and from no other; and your widely circulated journal has the true and just version of the matter in ascribing the professional demerits of this aspect of the Sarracenia problem to myself. The only co-operator with me in the matter is a layman, a gentleman of Irish family, and distinguished, which figured in the history of the second Charles of England, Mr. John Thomas Lane, Lanes Park, Tipperary, Ireland.

On the 17th of April, 1861, Mr. Lane brought a Mic-Mac squaw to me, with the root of the sarracenia in her hand. Her testimony I have on oath that the medicine she brought was a secret with herself and old "Sally Paul," a centenarian, who had adopted her from childhood, thirty-seven years since, in Canada, where her protectress at the time was using the medicine, and among others had cured several soldiers in one of the regiments stationed in the garrison. Here, then, is the starting-point of the sarracenia, after its long and silent slumber of so many centuries. Poor old Sally went to her rest a few months ago, and but for the provident action taken when the small-pox had just commenced a fatal onslaught upon the people of this province, it might still have slept on, perhaps for ever, in the night of ages, until the general wreck of all things here below. Mary Anne Ferris, the squaw who placed the medicine in my hands, gave her statement to me on oath, that Mr. Lane and myself were the two first white men to whom the secret had been divulged, and that Sally Paul was then curing the small-pox at Tangier, the present gold district of Nova Scotia, where it was raging.

A few days after I had occasion to try it in two cases, in which I at once recognised its wonderful characteristics,—its astonishing influence upon congestive ganglionic irritability, and all cerebral neuralgic development. I saw that its functional action was principally renal; but that by mercurial or antimonial influences, the tide of action might be made diaphoretic or cathartic at pleasure. There was no mistaking the specific power at the root of the remedial action! The disease was as powerless in the grasp of the sarracenic agency as ever victim was in the coils of the anaconda, or equally resistless and terrible boa constrictor. Every painful manifestation of suffering disappeared in a few hours. I saw parents in agony at the utter hopelessness of the apparently dying child,—in one case of the severest pyrectic temperature, where the heat was sensible to the hand at a distance of two feet from the body, and the medical attendant had left the case as irrecoverable,—in this and in every instance of trial all around the sick bed became sensible of the astonishing powers of the sarracenia in a few hours. Of course I did not allow the grass to grow under my feet; but in these, my beneficent intentions, I was obstructed by a perfect hurricane of opposition from my professional *confrères*, who could not endure that I should have such confidence in the unreliable statements and experience of a savage. Every day my position as medical officer of the institution became more precarious, until at length the governors of the dispensary plainly told me that I must abandon my public advocacy of the remedy, or give up my situation. My convictions upon the subject I utterly refused to abandon; but the further use of the sarracenia, after having witnessed its undoubted alexipharmic powers in about five-and-twenty cases, I was obliged to forego. Here, then, my hands were tied. I was not allowed to write about it in the press. One or two letters of mine had already appeared in the journals of this city. The Corporation had been invited to have the medicine tested in an express establishment for the purpose, and that it should be divulged upon consideration of a reasonable compensation to the poor squaw. But all in vain. At the instigation of the Board of Health, they had set their faces against it, and thus the matter stood. Sally Paul's pretensions now, after the action of the dispensary, began to have some weight with white folk, and even with her own tribe, who in general mistrusted her. Her own people came flocking to her, and many were the applications from the city. Mr. Lane was distributing the medicine far and wide, whilst I was obliged to look on in silence. Judge then of my astonishment, months after this, upon Mr. Lane coming to me with the intelligence that Mr. Miles had gone up that very day, he was informed, to obtained the secret of the cure from Sally Paul, and that with a bribe of a few pounds he had obtained it. The next intimation I had of Mr. Miles's action in the matter was his letter to the Epidemiological Society, totally ignoring the dispensary, and claiming for himself the *primum mobile* of its professional announcement! My treatment of small-pox with sarracenia commenced in April, 1861. Mr. Miles's letter to the Society appeared in November of the same year. At the time of my trials with it in April, Mr. Miles, an acquaintance of Mr. Lane, and in garrison here at the time, was daily meeting Mr. Lane, and asking him how Dr. Morris was getting on with the sarracenia. Small-pox was at this very time in garrison, under Mr. Miles's inspection, and soldiers had died of it under Mr. Miles's own eyes, and under treatment to which Mr. Miles must have been a party. If this gentleman possessed any experience of the medicine, how is it that he was not using it simultaneously with myself? He could not have used it in the garrison cases, for the soldiers had died. Had they died under treatment by sarracenia, Mr. Miles must have known it. Had he known it, he could not have written that letter to the Epidemiological Society. A glance at Mr. Miles's communications to The Lancet (August 25th and October 18th, 1862), shows us that he has no practical knowledge of the subject whatever. Dr. Richardson's case, quoted, corresponds precisely with the statements invariably given by myself in various quarters, and yet Mr. Miles arrogantly claims for himself all jurisdiction of originality and professional patronage in the proceedings.

Why, there is no complex mystery in the use of the sarracenia. Nothing can be more simple. The most obvious and unmistakable feature of the sarracenia as to taste is its decidedly "willow flavour," so like the "salicin" that any one would take its infusion for that of the alkaloid named. Whatever part of the sarracenia imparts to the tongue this willow flavour may be relied upon as the cure for variola. The roots, young or old, all exhibit this flavour, as also the rootlets, rhizoma, radicals, and even the loose flocculi of the epidermis, that so easily separate from the plant by friction. I have used them all in my cases equally with success. A drachm of any such portions of the plant infused in a pint of water until reduced one-half, and a wineglassful given every three hours all through the disease, whatsoever the symptoms, is all that is necessary to be done. This treatment is sure to give a good account of the patient. Stronger doses than this would have a tendency to make young patients vomit, and reject the medicine at a time when not a moment should be lost. There is no mistaking the sarracenia in its "habitat." Imagine half a dozen purplish-red, vegetable pouch-looking leaves, in shape like a young straight cow horn, inverted and attached by the smaller end to a root not unlike the sarsaparilla; the leaves or pouches disposed around the root in a whorl of six or more, and from the centre or axils of the leaves one or two rush-formed, glabrous scapes springing up, about eighteen inches, quite as smooth, hard, round, and dense as "the rush," but hollow and catheter-like; imagine this "rush-like scape" to terminate in a flower, with a browish, glossy calyx, three-lobed, a germ and stile, with stigma expanding over them, like a university cap, of fine terminal points at the margin; the petals, of striated crimson, overlapping the expanded stigma upon its marginal incurvatures; three concave, small bractea, corresponding with the calyx in colour, and in curvature, presenting in all an image or flower in contour quite mistakable at a few paces for a "camelia; attach a hood like a squaw's cap to the larger end of the cow-horn leaves aforesaid, and looking upwards to catch all moisture from the sky, and pass it into the recumbent horn; let the horns have upon their upper margin a broad fringe each, and tapering away to the shank and axil of the leaf, or small end of the horn, like the squaw's leggings, and you have a sufficient idea of the sarracenia plant as it grows in the bogs, that if you chance to fall in with, you never can mistake.

Any one now who will compare my statements with the following language of Mr. Miles in the Lancet:—"It was during the last outbreak of the disease amongst the Indian settlements in Nova Scotia that the decoction of the root achieved its greatest triumph; that the faith in its efficacy became wide-spread and general,"—will at once discover the real features of this question, as truth must ever represent it. It was precisely at this moment that H.M.S. *St. George*, with his Royal Highness Prince Alfred, arrived here from the W.I. station, when Mr. Lane, anxious for the son of our beloved Sovereign, sent a package of the medicine on board to prevent any possible casuality from the disease. When I first saw Mr. Miles's letter to the Epidemiological Society, so many months after the sarracenia had undergone its probation here, and the scourge had passed from it, I was disposed to consider it but a generous impulse of Mr. Miles to the *rescue* of one to whom he was an utter stranger; but Mr. Lane could not help thinking it an intention to *supplant*, as no reference was made to the initiatory action by me at the dispensary. The communications of Mr. Miles and his brother since then in the Lancet have completely enlightened me, and I am sorry to find the spirit of charity in this direction quite frozen within me.

The sarracenia antidote I have no doubt is the providential agent to deal with the contagious element *en masse*,—to extinguish contagion by some specific disorganizing process in a way and by a power utterly disproportionate in its infinitesimally minute concentrativeness, just as the fraction of vaccinia pervades from a needle's point the entire system. It is true that the variolous element, under sarracenia, is expelled with accelerated current by the renal outlets, and is in this way eliminated from the system as "the saccharine" is in diabetes. But it is something more than this. This very urine itself should communicate variola were it all variolous; but the virus is gone, effete, and impotent,—the sarracenia has extinguished it. I have no hesitation in pronouncing Sarracenia purpurea to be the remedy for variola, varicella, rubeola, lepra, psora. I have used it with unqualified success in these cases. It not only *relieves*, but it *extinguishes*, and, what is more, in any given instance it is sure to emancipate from subsequent abnormal developments, as a general rule. In psora, lepra, and skin disease, I have little or no trouble, and I find a liberal use of the chloride of calcium a very decided auxiliary and quickener of the sarracenic virtues in skin treatment.

It would be just as well to mention here that any person carrying with him the root of the sarracenia may *chew* his way with perfect impunity through small-pox wherever it may stand in his path. I sincerely hope it may be so with plague, Asiatic cholera, or any other scourge.—*Lancet, December* 6, 1862, *p.* 638.

4.—CASES OF SMALL-POX TREATED BY THE SARRACENIA PURPUREA.

By Dr. H. Chalmers Miles.

[As the Sarracenia Purpurea, or American pitcher plant, may now be obtained in any quantity in London, it is to be hoped that all practitioners who have the opportunity, will give the remedy as early and ample a trial as possible.]

In furtherance of my desire that the profession should have the earliest information concerning the remedial action of the sarracenia, I beg to place on record a few brief notes of four very interesting cases of variola treated by the root, which have been kindly forwarded to me by Mr. S. K. Burch, of Shaftesbury-crescent, Pimlico; and shall be very glad if such cases are permitted occasionally to appear in the Lancet, until such time as in my opinion the effect of this medicine on small-pox is placed fairly and intelligibly before the profession and the world. I make no comment on the cases narrated, while the practice of publishing only "*sample cases*" is altogether ignored.

Mr. Burch mentions that he did not see my paper until ten days after it was published; but that on the 31st of October he commenced the administration of the sarracenia to Mrs. A., who had several days previously been attacked with "distinct small-pox of very severe type." On the 31st, when the sarracenia was first given, "the pustules on the face were so full that although distinct, a pin's point could scarcely be placed between them. The secondary fever was very high." On seeing her the next day (the decoction of the root having been given at the strength recommended, and at the prescribed periods), Mr. Burch found "the change very great, all the fever had left, the pustules on the face were half the size and of a dark-brown colour, and the skin was to be seen between them, while on the legs they were entirely depressed and flattened. Many of them curiously rose again for one day, then flattened and desiccated. On the 4th of November they all dried up and fell off. The patient remarked that she felt better after every dose. The sarracenia did not act as a diuretic *after* the first day; but the tongue rapidly cleaned. Mrs. A., was delirious the first night she took the decoction, and her husband and nurse had great trouble in keeping her in bed, as she was so anxious to get at the medicine." There were no marks of pitting in any of the cases now related.

Case B.—A lady, who lives in the same house as the former patient, took the sarracenia in mild doses as a *preventive;* but on the 11th of November the eruption (varioloid) began to appear, though in a very slight form, about thirty or more pustules showing themselves in different parts, except about the lips, where they were confluent, and rapidly becoming purulent; they desiccated, so that on the 15th she was quite well.

Case C.—A nurse in the same house also took the decoction as a preventive, and had variola in a still lighter form, and was ill only a few days.

Case D.—"Sent for me on Nov. 14th. The varioloid eruption was beginning to appear about the face, &c. It soon became confluent in many places, but about the body the pustules were distinct. On the 21st, having given the patient no medicine besides the decoction, there were no signs of desiccation or pitting; the pustules were purulent, but not filled out as they usually are on the eighth day; those on the legs were of a purple colour and small. She has been slightly delirious on several nights, but the fever has not been high. I told her to take the medicine regularly, when she said there was no fear of her not doing so, as she quite longed for the time to take it. On the 22nd the pustules began to dry up, and on the 27th were falling off rapidly; where they have done so there are no signs of pitting."

Each of these patients had been vaccinated. About Case A there is an additional circumstance worthy of mention. "The lady was between three and four months advanced in pregnancy. During the two days prior to commencing the sarracenia she hardly felt the child, and thought it dead, and also had a slight bloody discharge. After taking the decoction two days, she felt the child as strong as ever. This lady has been prematurely confined (between six and seven months) on two previous occasions."

Mr. Burch concludes by remarking—especially with reference to the last case—"Although the sarracenia has not acted quite so rapidly as I expected, still it has succeeded wonderfully, and will be a great boon. I am recommending all my medical friends to try it, and shall be happy to give you any further information, as I think the employment of the sarracenia purpurea in small-pox cannot be too much known."—*Lancet, Dec.* 4, 1862, *p.* 615.

5.—ON THE CONNEXION BETWEEN GOUT AND SOME ERUPTIVE SKIN DISEASES.

By Dr. George Ross.

[No very definite ideas are held by the profession on the subject of the connexion between gout and some skin diseases, yet the fact of this connexion is undoubted. Dr. Garrod gives a brief notice of such cases in his excellent "Treatise on Gout." Dr. Graves, of Dublin, relates a singular case of gouty inflammation of the nose, and Sir Henry Holland states, that he has often noticed the connexion between gout and psoriasis; but notices of cases of this nature are scanty.]

The gouty diathesis may exist for many years without any actual fit—without any positive manifestation in the great toe or other joint, of the slumbering flame. *Latet anguis in herbâ.* The venom is under the serpent's tongue, though the joint is not yet bitten. This "latent gout" is common enough, and explains numerous maladies, the causes of which seem to be otherwise unintelligible. It requires, however, considerable experience among the class of patients usually affected with gout, to be able to detect it—to divine that peculiar constitutional cachexia, which, until it breaks out in its own specific character, is quite Protean in its manifestations. What has been recently called, and appropriately too, alcoholism, is intimately connected, in some of its forms, with latent gout; yet the two conditions are not identical, and pursue different courses. The alcoholic diathesis and the gouty diathesis are distinct.

Hyperæsthesia and Prurigo.—Gouty people are liable to a variety of cutaneous affections. I may premise that the gouty swelling itself is an erythema, not an acute inflammation in its ordinary sense, but a transient erythema of the cutis, attended with œdema of the subjacent tissue. Although the presence of gout usually determines this erythematous inflammation to the neighbourhood of a joint, yet it may also induce a morbid condition in other parts of the cutaneous surface. One of the most troublesome and well-known states of the skin, incidental to the gouty constitution, is hyperæsthesia. This increased sensibility is usually experienced between the toes, but it also prevails on different parts of the body. I have now under treatment, a lady who has been for many years a sufferer from gout, and who was lately so irritated by the intolerable itching between her toes that she scratched off the skin in her efforts to obtain relief. The hyperæsthesia, in this situation, is almost as common as the erythema which it precedes.

Not long since I was called to a gentleman who woke in the night with a sense of heat and feverishness, oppression at the præcordia, anxiety, and a troublesome itching and tingling in his palms and various parts of his body. It was the peculiar and intense formication in his palms that gave him most concern; it being a new symptom. Nothing was visible, and, in fact, at the time I saw him the itching had nearly left; but in a day or two he had a gouty attack.

This hyperæsthesia often runs into a more palpable form of disease—viz., prurigo. This affection not unfrequently attacks the vulva and anus, and is, of course, when in this situation, exceedingly troublesome. I have seen these affections coincident with undeveloped gout several times; and the most effectual cure is nature's own—an outbreak of the mischief in the great toe. Sometimes this object cannot be attained, and, in that case, a protracted pruritus will continue, unless assuaged with remedies adapted to remove the gouty poison. These cases of prurigo are always associated with disturbance of the digestive functions—mal-assimilation, flatulence, anorexia—sometimes gastrodynia, and frequently constipation, with much mental disquietude and depression; in short, the constitutional symptoms are more marked in these instances than in simple cases of the disease.

Erythema Nasi.—I have given a local appellation to a frequent form of gouty erythema. A gentleman, about fifty years of age, consulted me not long since, in a state of considerable annoyance, concerning an unexpected redness and swelling of his nose, extending over his left cheek and eyelid, the latter being puffy, as if erysipelatous. The nose was about half as large again as an ordinary nose, and was especially tumefied about the tip. It was hot and tense, with a pricking sensation, but not particularly painful—in short, it was a gouty nose. The affection lasted about four days, at the end of which time, the organ gradually resumed its ordinary dimensions and colour. As an explanation of this case, I may state two things—first, that the gentleman is subject to an inveterate lichen; and secondly, that, although generally, as I was assured, a most abstemious man, yet he had, for weeks past, indulged, with some freedom, in malt liquors of a superior quality, with the object of improving the tone of his system. He had certainly attained a better result than he had anticipated. This gentleman has never had an actual fit of the gout, although he has been suspicious of its presence, and has recently had flying pains over his body.

This case calls to mind that reported by Dr. Graves, to which I have just alluded. Dr. Graves's case, however, was periodic, coming on daily for a considerable period—not a very characteristic evidence of gout;—mine was not, but simply ran its course as a gouty erythema might be expected to do.

Lichen and Eczema.—I will now give a few instances of the complication of other cutaneous maladies with gout. I have already related a very peculiar and interesting case of lichen, breaking out coetaneously with an attack of gout. The beginning and ending of that eruption with the gouty inflammation was very remarkable. I have seen the gentleman casually since the publication of my last lecture, and his skin is clear of any eruption. I will now detail the particulars of one of the earliest cases in which I noticed this complication.

I was attending a patient—an aged female—who was suffering from an old ulcer of the leg ; suddenly it inflamed, and discharged a thin sanious fluid ; the inflammation extended to the ankle-joint, and on inquiring into the cause of such an unexpected attack, she directed my attention to the small joints of her left hand, which were swollen with gout. The existence of the ulcer had attracted the gout to the part. This was no new thing to her. Besides the gout, however, a lichenous eruption had broken out upon the leg ; the papules afterwards coalesced, and superficial ulceration took place. Here was a case of lichen agrius, which would degenerate into eczema, if left to run its own course.

Psoriasis.—The existence of psoriasis as consequent on, or coincident with, the gouty diathesis, has also been noticed. I do not think, however, that psoriasis is so immediately dependent upon the gouty constitution, as the affections I have already described. It is a disease that forms slowly, and perhaps is relieved, rather than caused, by an attack of gout. Undoubtedly the skin of a gouty patient is often dry and harsh, excepting during a paroxysm, and therefore predisposed to this form of disease. I have, however, cited a sufficient number of cases that have come under my own observation to show the frequent complication of eruptive diseases with gout.

Tophaceous Deposits.—Although these deposits have not been hitherto regarded in connexion with diseases of the skin, yet some notice of them must not be omitted when treating of the gouty constitution. These deposits consist of the urate of soda, and vary in consistency from a semi-fluid to a solid state. They are usually found in the neighbourhood of the small joints, but are also, as Dr. Garrod first pointed out, often observed on the helix of the ears of gouty patients. When seen in this situation, they are white in colour, and about the size of a pin's head or larger, the helix itself being occasionally erythematous, and injected with blood vessels. They cannot be easily mistaken for any other affection of the skin. The ears, in short, are liable to gouty attacks, like the nose, as I have already described.

Is there a common cause of these affections? It is generally difficult to determine the proximate cause of a febrile attack, or, indeed, of any disease. What is the necessary condition of the system, or of either, or any of its organic constituents, that induces typhus, or cancer, or a common catarrh? In this sense, what is the cause of prurigo, or lichen? We find eruptive disorders often associated with irregularities of the digestive functions; and this is particularly the case when they supervene upon a gouty diathesis. Whether this disturbance throws into the blood some irritating principle, or exerts a special poisoning influence upon the nervous system, or whether the enfeebled tone of the digestive organs be a consequence of systemic changes, it would be mere guesswork in the present state of our knowledge to presume to determine. The human body is a complicated piece of machinery, and one wheel fits into and turns another so admirably that it is difficult to say where motion begins or ends. Health is the result of a succession of changes, involving action and reaction.

It must be observed, however, that all eruptive disorders commence with a febrile stage, and when they have become chronic, are liable to excerbations. This febrile state is due to a specific condition of the system. Be it an excess of acid in the blood, a change in the relations of its component parts, or an impression upon the nervous system of a reflex nature, whatever it may be, such a condition exists. I am inclined to think, however, that an acute attack of an eruptive disease is an effort of the system to depurate itself of some noxious element. I find that the cause inducing a fit of the gout will also induce or modify a cutaneous affection. We know that the blood in gout is charged with uric acid, and it is proved to demonstration, that this principle constitutes the characteristic evidence—whether the essential cause, I say not—of the malady.

Is uric acid in the blood an irritant of the skin? This question must be answered in the affirmative, if we consider for a moment, that the local manifestation of gout itself is, in fact, an erythema of the skin. This is its primary and characteristic manifestation. Then it has been stated that uric acid has been actually secreted by the skin. Several instances have been recorded of a whitish powder being found on the skin of gouty patients, and Dr. Charles Petit has asserted that he has detected —or rather Mr. Henry, who performed the analysis—urate of soda in the cuticular secretion, taken from several parts of the body of a gouty patient. These observations have been satisfactorily confirmed. Another salt, the oxalate of lime, also a characteristic of the gouty diathesis, has, however, been found in the perspiration.

The secretion produced by artificial irritants has afforded more direct testimony. Dr. Garrod has examined the serum from a blistered surface, in several instances, and has discovered that it contains uric acid, except when the blister had been applied to parts that were actually inflamed.

It may, therefore, be reasonably inferred, that the salts characteristic of gout, do determine towards the skin, and that in some cases, as in eczema, uric acid might be detected in the secretion from the diseased surface. It is probable, however, that if the gouty inflammation, as in an instance which I have previously related, ran high, the characteristic acid might not reply to reagents. It would not follow, therefore, that because uric acid did not exist in the secretion from an inflamed surface, that it was not actually irritating the skin; for Dr. Garrod has shrewdly remarked that the gouty action, in a part, seems to have the effect of destroying the uric acid.—*Medical Circular, March,* 4, 1863, *p.* 116.

6.—ON THE TREATMENT OF ACUTE RHEUMATISM BY NITRATE OF POTASH.

By Dr. W. R. Basham, Physician to the Westminster Hospital.

[A previous article on this subject by Dr. Basham, will be found in *Retrospect*, Vol. xix, p. 20. Twelve years' further experience have confirmed in every respect the principles then enunciated.]

The most striking feature in an attack of acute rheumatic fever is the intensity of the local inflammation of the joints, and the special tendency that co-exists for an inflammatory attack to invade the heart, either by inflammation of the pericardium, exocardium, or the valves of the left ventricle. There is present in acute rheumatic fever a very striking and exceptionable state of the blood, marked by the predominance or great excess of the fibrin of the blood. The average proportion of fibrin in healthy blood is about two parts in a thousand, but in acute rheumatism it often exceeds six : nor is this all—the saline constituents play an important part in the composition of this fluid, and ordinarily may be estimated at about twelve parts in a thousand. But in acute rheumatism the proportion descends as low as eight—or even seven. Here are two important pathological facts,—the absolute excess of fibrin, and the absolute decrease of the salts. Now, although we are not certain that we can demonstrate the positive relation which the saline constituents bear to its fibrin, yet by analogy we can infer that some important connection exists between them. Thus, the fibrin of the blood will not separate or coagulate, as it is called, if blood be received into a vessel containing certain neutral or even alkaline salts. But the most important fact is that observed by Dr. Stevens, in the state of the blood in a person who, by mistake, had swallowed an ounce of nitrate in solution, mistaking it for Glauber's salts. In those days it signified little what symptom a patient exhibited, or what accident he might suffer, the first act was to open a vein. This man, who had swallowed this large dose of nitre, was bled, and, to the astonishment of Dr. Stevens, the venous blood came from the vein almost as bright as arterial, and continued fluid, without exhibiting the usual characteristic of spontaneous coagulation.

Now, let me apply this fact to the treatment of acute rheumatism by nitrate of potash. First, we know that in the middle of the last century large doses of nitrate of potash were successfully given in this disease. Secondly, we now know that nitrate of potash, circulating in the blood, prevents the separation or coagulation of the fibrin. And, lastly, in acute rheumatism we know that there is not only a large excess of fibrin in the blood, with a diminution of the salts, but we know, also, that there is a special tendency for this fibrin to escape from the circulatory fluid, and to form the exudative products which are thrown out in the inflamed tissues, and which constitutes such an element of danger when it is poured out on the pericardial or exocardial surfaces, or forms deposits on the aortic or mitral valves. It is a fair and rational inference, then, that a salt which can prevent the coagulation or separation of the fibrin when present in the circulatory fiuid in a person in comparative health, will operate in a similar manner where the blood is contaminated by excess of fibrin.

The chances of inflammatory fibrinous products being poured out are thus lessened, and time is obtained for the natural processes to reduce the excess of fibrin and to restore the blood once more to the standard of health. Such, then, is the reasoning applied to the employment of nitrate of potash in acute rheumatism. But recollect that these doctrines are applicable alone to that form of rheumatism which is usually denominated acute, and which is always accompanied by manifest local conditions of inflammation about the joints—principally of the hands, elbows, shoulders, ankles, and knees. This treatment is not adapted to those forms of rheumatism which are called either chronic, or muscular, or fibrous, and characterised chiefly by wandering pains in the limbs, or various parts of the body, and which are never accompanied by any disposition to local redness or tumefaction. If the principle be steadily kept in view on which the nitrate is employed, there is little chance of the error of employing it in those forms of rheumatism which are not dependent on, or connected with, a hyperinotic state of the blood. I cannot forbear noticing here an objection which has been raised by Lehmann, to what he conceives to be a pharmacological error on the part of those physicians who believe in the antiphlogistic power of the nitrate of potash. He says it is difficult to draw the conclusion that nitre can prevent the formation or augmentation of fibrin in inflammatory blood simply because coagulated fibrin is soluble in a solution of that salt. In such a conclusion I desire to express my perfect concurrence. I have never adopted such reasoning : I have always believed that the nitrate of potash does not lessen the development or formation of fibrin in the blood, or in any way regulate the quantity formed. The opinion which I have ventured to express, and the explanation I have put forward to interpret the action of this salt in inflammation, is based on the following facts :—

1. Nitrate of potash, when present in the blood of a living animal, retards or suspends the separation of the fibrin.

2. In inflamed blood, there is a considerable increase in the proportion of fibrin, and the essence of the inflammatory process consists in the exudation or escape of this constituent of the liquor sanguinis, and its deposit in the tissues.

3. The presence of nitre, even in small quantities, in the circulating fluid, suspends this disposition of the surplus fibrin to form exudations; while other agents, or even natural causes, may be operating to reduce the proportion of fibrin, and bring it within healthy limits.

The first is a physiological fact, proved in its direct applicability to man by Dr. Stevens' case of the non-coagulability of the blood of one who had swallowed a large quantity of nitre by mistake. The second rests partly on chemical, partly on pathological proof, and cannot be disputed. The third rests purely and solely on clinical observations: it is, to a certain extent, hypothetical, and incapable of direct proof—but it is based on the constant sequence of marked and striking exemptions from the usual consequences of the inflammatory process, and which are not observed to follow with so much certainty or regularity when other and different means are employed. Lehmann further states an objection to the assumed properties of nitrate of potash, founded on the quantity of nitre usually administered in the twenty-four hours as being inadequate to the solution of the fibrin present in the blood. Assuming the quantity of blood to amount to twenty pounds, and that the fibrin amounts to three parts in a thousand, then the quantity of fibrin would be somewhere about 300 grains, and he contends that 120 grains of nitrate of potash would be necessary to dissolve this quantity, whereas not more than ten grains are administered every two hours, and the whole quantity in 24 hours would not exceed 100 or 120 grains. And as the salt passes off so rapidly by the kidneys, it must be prevented from accumulating in the blood. But, in answer, it may be stated that patients constantly take 480 grains in the twenty-four hours when freely diluted. That the salt passes rapidly off by the kidneys is well known; but by frequent repetitions of the dose of the salt, a sufficient quantity is constantly kept present in the circulating fluid, not to dissolve all the fibrin, but to retard or prevent any excess forming inflammatory exudates when a state of hyperinosis exists. This author further states that it would be very singular and inexplicable why other salts which equally possess the power of dissolving fibrin should not be classed with nitre as special antiphlogistics; for instance, alkaline carbonates, which are remarkable for preventing the coagulation of the blood. It is not without hesitation that I venture to remind so distinguished a physiological chemist, that other salts are equally efficacious, and that the alkaline carbonates have been proved by clinical observations to possess this power of suspending or modifying the exudation of fibrin. But many of these salts have special physiological properties, some acting as purgatives, as the sulphate of soda; others as diuretics and purgatives, as the acetates. The alkaline carbonates have also been proved to exercise modifying properties; but the objection to their employment rests, as has already been shown, on their marked interference with the functions of the stomach when given in sufficient quantity to exercise an influence over inflammatory exudations. The nitrate of potash in these respects possesses certain negative properties which adapt it for administration in preference to other salts.

When first I commenced the employment of the nitrate of potash in acute rheumatism, and, indeed, up to the period of the communication to the Medical and Chirurgical Society on this subject, the nitrate was used in co-operation with other remedies, such as calomel and opium, antimony, and even colchicum. But I have long since ceased to employ any of these latter remedies, experience having convinced me that the real efficient agents in this acute disease are opiates and salines. Brisk purgatives in some cases are desirable at the outset of the treatment; but, subsequently, they are not necessary.

It is not a little singular that a remedy so efficient in rheumatism, so unequivocally remedial, tested by the experience of many hundred cases which are uniformly treated here on one definite plan, should scarcely yet be recognised by the profession as specially applicable to the treatment of acute rheumatism. No doubt it takes a long time for a remedy to win its way into the confidence of the profession. It is not sufficient that the principles on which it is employed are sound and intelligible. It must yield in the hands of each practitioner similar and uniform results; and, through the testimony of the many, it at last secures for itself a permanent confidence in its utility. It is a century since a most distinguished metropolitan physician asserted its pre-eminent efficiency in acute rheumatism; and there have not been wanting physicians in recent times to bear witness to the success of cases treated by this salt. In France its efficacy has been recognised by Gendrin, Martin Solon, Bonnet, and others, and yet it is far from being accepted in this country and employed as universally as its utility demands. And why is this? I think the explanation is not difficult. Sufficient attention has not been paid to the fact, that it is only in the acute inflammatory rheumatism—the form of disease which is usually termed rheumatic fever—that this salt proves efficacious. Acute inflammatory symptoms in the joints, with febrile disturbance, are the indications for its use. But the term "rheumatism" is, unfortunately, of very wide and inexact application. It comprises many forms of very different morbid processes. Wandering pains in the limbs are called rheumatism; muscular pains are called rheumatism; aching pains from whatever cause, the sequel of malaria, of syphilis, of mercury, all are recognised by this common term. Certain inflammatory conditions affecting the joints, more particularly of females,—a true arthritis, sometimes called rheumatic arthritis,—are examples of the inexactness with which one common term continues to be applied to very different morbid conditions. And here is found the explanation of the apparent failure of nitrate of potash in the treatment of rheumatism. It has been tried and used in cases in which it is totally inapplicable. I have frequently had the remark made to me by practitioners, that they have used the nitrate and found no advantage in its action. Upon inquiry, it has always been proved that the cases were not acute rheumatic fever, but some form of rheumatism, the pathology of which differed materially from that of the acute disease now under consideration. I continue, however, to receive the testimony of many able and distinguished physicians, who have carefully employed the nitrate in appropriate cases, that its efficiency is equal to what Dr. Brocklesby claimed for it, and that the results to which I called the attention of the profession in 1848 have been confirmed; and that cases thus treated are, to a great extent, exempted from cardiac complication, and that, if attacked with heart symptoms, the inflammatory process is mitigated and controlled; and also that the duration of the acute stage and the intensity of the inflammatory symptoms are diminished; and the convalescent period, which every one who recollects when acute rheumatism was treated by bleeding, calomel, antimony, and opium, knows to have been most tedious and protracted, at the present time is seen to be moderate and limited, the patients quickly recovering their vigour and bodily energy. One cause of this more favourable stage of convalescence arises from the diminished destruction of the blood corpuscles which occurs in cases of acute rheumatism treated by the nitrate of potash. That peculiar pallor and exsangueous look which was so characteristic of the convalescent stage of patients treated by bleeding and antimony, is rarely seen to the extent formerly witnessed. There is thus less reparative work to be done. Nutrition and stimulants in moderation more quickly restore the blood to a healthy and invigorating condition; and the healthier, even ruddy, aspect, which quickly follows such a treatment, is the best proof that these principles of therapeutics are both practically and theoretically sound. Let me, lastly, ask your attention to a few brief remarks on the indications for wine and stimulants in these cases of acute disease. I believe that no better or safer guide can be found than the state of the pulse interpreted by the sounds of the heart. I say "interpreted by the sounds of the heart," because the condition of the pulse must be judged relatively—not absolutely. The want of power, the feebleness, the compressibility of the pulse cannot be estimated correctly by the finger alone. A pulse, moreover, may feel full and soft to the sense of touch, and yet the vital energies are failing, and, if not supported, will succumb. But, if the stethoscope be employed to verify the suggestions of the pulse, there is little liability to error. A pulse may be very small and compressible to the finger, and yet the impulse of the heart be good, and the sounds clear and distinct. In such a case no stimulants are needed. But the pulse may, to our senses, be full, and apparently with some resistance to compression; but the impulse is imperceptible, and the sounds feeble and indistinct. Here, wine is needed, and, in the course of a few doses, you may listen at the præcordium, and you will have sufficient reason, in the more vigorous sounds now heard, to be convinced of the correctness of the indication which the stethoscope has afforded. There are other circumstances which will also guide you in the administration of wine. A failing temperature of the skin, a tongue becoming red and dry, confusion of thought, and a tendency to incoherency, all are suggestive of failing energies, and the propriety of a liberal allowance of stimuli. But the stethoscope will give warning long before the latter signs are present, and will enable you to keep the patient up with far less quantity, and at less hazard, than when the latter indications have tardily suggested that stimulative measures are imperatively required.—*Med. Times and Gazette, Dec.* 20, 1862, *p.* 651.

7.—ON ACUTE RHEUMATISM.

By Dr. Henry Wm. Fuller, Physican to St. George's Hospital.

[After relating an interesting case of Acute Rheumatism, Dr. Fuller remarks that his aim in the treatment of this disease is always to induce alkalinity of the urine as speedily as possible.]

Experience has shown me that as soon as a full alkaline action has been induced, as indicated by alkalescence of the urine, the heart is safe, and no fear need be entertained of its suffering from the irritation of the rheumatic poison. In no single instance has a patient of mine been attacked with endocarditis or pericarditis after the urine has shown an alkaline reaction. But in a case of acute rheumatism in which febrile action runs high, and the urine is loaded with lithates, it is not an easy matter to induce alkalinity of the urine. Alkalies may be given in moderate quantities without producing the slightest apparent effect on the urine, without in any degree modifying the general symptoms of the disease, and without affording the slightest protection to the heart. In this instance, as in other cases, half measures are useless, or worse than useless; they fail to effect the object in view, and yet suffice to lull the patient and his medical attendant into a false and unwarrantable security. To be of any service, alkalies must be rapidly pressed to the fulfilment of the object for which their administration is had recourse to. They must be given in doses proportioned to the intensity of the mischief they have to remedy, and must be repeated as frequently as occasion may require. Ordinarily I find that two drachms of some alkaline carbonate or its salts, if repeated every three or four hours, will suffice to induce alkalinity of the urine within twenty-four hours, and this therefore, is the dose with which I generally begin, taking care to add a few grains of citric acid, with a view to induce effervescence, and render the medicine less unpalatable. Sometimes, notwithstanding these full doses of alkali, the urine will remain acid for thirty-six or forty-eight hours; but this is only in very aggravated cases, and when the bowels are costive, and it must be regarded as an exception rather than as the rule.

When once alkalinity of the urine has been induced, the dose of the alkali should be greatly diminished, and this may be accomplished either by reducing the quantity of the salt in each dose which is administered, or by giving the original medicine at longer intervals. Ordinarily I adopt the latter plan, inasmuch as it answers just as well as the former, and causes less annoyance to the patient.

When the urine is alkaline, and has remained so for two or three days, notwithstanding the great decrease in the quantity of alkali exhibited, and when at the same time the skin, though warm, and possibly perspiring, is no longer hot as during the full febrile stage of acute rheumatism, and when, further, the pulse, though still more frequent than natural, has lost its force and fulness, then, notwithstanding the furring of the tongue, the administration of cinchona will prove serviceable. If, under the circumstances just named, the tongue remains much furred, the probability is that active purging will be required; but the exhibition of bark will not thereby be contraindicated. You have seen this fact exemplified so often, and have so frequently had your attention directed to it, that I feel almost ashamed of alluding to it again; but it is a point on which erroneous views are commonly entertained, and therefore it cannot be too earnestly or too repeatedly pressed upon your notice. The bark effects a double object. It sustains the patient's strength without heating or exciting him, and it appears in some way to modify the morbid actions whereby the continued formation of the rheumatic poison is brought about. At an earlier stage of the attack it seems to increase the general derangement, and acts prejudicially; but when the patient has been five or six days under treatment, and the results above described have been produced, I have seldom seen it otherwise than beneficial. The only precautions necessary will be found to be, that throughout the period of its administration a full action of the bowels should be secured daily; and that until the tongue has thoroughly cleaned, and the pains have subsided, sufficient alkali should be given with it to keep the urine slightly alkaline.

The preservative influence of alkalies over the heart was first pointed out in my work on Rheumatism; but at the time when the first edition of that work was published, I scarcely dared anticipate the marvellous success which extended observation has proved to attend their administration. I then stated that they exercised a sedative influence over the heart, and prevented the extension of inflammation to it; but the facts which I had then collected were not sufficiently numerous to enable me to affirm, as I now do, with perfect confidence, that, if duly administered, alkalies will entirely prevent the access of cardiac mischief. You are all aware that they have protected the heart from attack in every case which has come under my care in the wards; but you are not probably aware that this same immunity has been observed in the cases under my charge ever since I was appointed physician to the hospital. No patient of mine whose heart on admission was free from mischief has left the hospital with that organ damaged. Our resident medical officers and our successive medical registrars can all bear testimony to this fact, and its importance can scarcely be over-estimated. When one considers the vast amount of suffering entailed by rheumatic affections of the heart—when one looks around our wards and finds them filled with patients suffering from palpitation, asthma, and dropsy, resulting from disease of the central organ of the circulation—disease which originated, perhaps years ago, in an attack of acute rheumatism,—I say, when one reflects on these facts, one cannot feel too grateful to Almighty God for having placed in our hands the means of preventing so much suffering, and of preserving thousands from a premature grave. Let me briefly refer to the statistics of the subject, and you will then become aware how great is the value of this power of preventing the accession of cardiac disease. The statistics which I will lay before you are recorded in my work on Rheumatism and Rheumatic Gout, and relate to cases which occurred in St. George's Hospital during the period of my registrarship, and before the employment of the full alkaline treatment:—From the 1st of January, 1845, to the 1st of May, 1848, there were admitted into the hospital, under the care of the then physicians, 246 cases of acute rheumatism; and amongst these there occurred 12 instances of pericarditis alone, 27 instances of endo-pericarditis, and 75 cases of endo-carditis alone; in other words, the heart was damaged to a greater or less extent, as the result of the attack for which they were admitted, in no less than 114 patients, or in 1 out of every 2·1 cases; its pericardial covering being the seat of inflammation in 1 out of every 6·3 cases, and the endo-cardial membrane covering its valves in as many as 1 out of every 2·2 cases. Nor are the results of our experience at St. George's inconsistent with those of other observers. Dr. Latham, at St. Bartholomew's, and Drs. Basham, Wm. Budd, Taylor, and others, have arrived at nearly the same conclusions; so that you may regard the fact as admitted, that under ordinary circumstances the heart is damaged to a greater or less extent in at least one-third of all cases of acute rheumatism. Contrast this fact with that which I just now brought before you—namely, that by the judicious employment of alkalies this cardiac affection may be almost wholly if not entirely prevented, and you will then be in a position to judge of the vast importance of this discovery. No step in advance at all equal to it has been made of late years in practical medicine; no change of treatment of any disease can be pointed to which will bear comparison with it in its effect of warding off human suffering and obviating the tendency to untimely death.

There is yet another point in relation to the case of E. H., to which I would specially direct your attention. I refer to the albuminous condition of the urine. On her admission into the hospital, the urine was extremely albuminous, becoming almost solid in the test tube on the addition of nitric acid, and its specific gravity was 1033. After she had been three days under treatment, my clinical assistant reported that it was alkaline and scarcely albuminous, and on the sixth day it was "abundant, pale, clear, alkaline, no longer albuminous, and its specific gravity 1015." Here, then, we have evidence of an albuminous condition of the urine due to renal irritation and congestion caused by the action of the rheumatic poison—an occurrence not *a priori* improbable, but one nevertheless not commonly met with. I wish you to note the fact, and bear it constantly in mind; as, if you are not alive to the possibility of such an occurrence, and you chance to discover a large amount of albumen in the urine, you may jump to a wrong conclusion respecting the nature of your patient's malady. In several instances within my own knowledge in which albumen has been discovered in the urine at one or two examinations under these or similar circumstances, patients have been told by their medical attendants that their kidneys were diseased, when in truth these organs were perfectly sound, though for the time functionally deranged. A few years since Dr. Parkes undertook an investigation into the state of the urine of patients admitted into University College Hospital suffering from a variety of severe disorders; and in the Medical Times and Gazette for 1859, where he has recorded the results of his observations, he tells us that albumen makes its appearance in the urine for a longer or shorter period in a large proportion of all acute disorders. My own experience leads me to endorse this statement, and to warn you against being misled by the existence of albumen in the urine at one or two examinations, more especially when febrile action is going on in the system, and the specific gravity of the urine is high. Such a condition of urine is by no means uncommon under the circumstances named in persons whose kidneys are organically sound. But in no class of patients will you see the fact exemplified more strongly than in those who are suffering from acute rheumatism, and in none certainly would you be more likely to be misled if you were unaware of the possibility of such an occurrence. In the case of E. H., which we are now discussing, the amount of albumen was very considerable; and in a case which occurred in University College Hospital, and which is recorded by Dr. John Taylor in the twenty-eighth volume of the "Medico-Chirurgical Transactions," "the urine was not albuminous in the patient on admission, but in a short time it became so highly charged with this principle as to assume a solid form when heated, and its specific gravity at the same time amounted to 1050. After a few more days, no trace of albumen remained, and the urine had assumed its ordinary appearance and density." Remember, then, that the presence of albumen in the urine during the course of acute rheumatism is not necessarily indicative of organic disease of the kidneys; that it may be, and, if the specific gravity of the urine is high, it probably is, due to temporary congestion of the kidneys; that no special treatment is ordinarily needed for its relief, but that alkalies, by getting rid of the materies morbi, and thus removing the cause of local irritation and congestion, will usually suffice to rid the patient of its presence in the course of a few days. Nevertheless, in any case in which the skin is dry, a vapour or a hot-air bath should be resorted to; and if the urine contains blood and gives evidence of intense renal congestion, the aid of dry cupping on the loins may be invoked with the view of more rapidly subduing the mischief.

The administration of soda is not in accordance with the theory which attributes rheumatism to a deficiency of the salts of potash in the system; neither is it in harmony with the practice of those who admit the value of alkalies, but confine themselves to the administration of potash, as forming compounds excessively soluble, and therefore capable of being easily eliminated from the system. But, in truth, I do not admit the theory referred to; neither do I consider that the sole object to be attained in the administration of alkalies is the neutralization of acid, and the formation therewith of compounds capable of being easily eliminated from the system. The virtue of alkalies, I believe, depends upon the power, 1st, of acting as restorers of the alkaline condition of the system; 2nd, of assisting to maintain the solubility of the fibrin, and thereby preventing its deposition on the valvular apparatus of the heart; 3rd, of acting most powerfully as sedatives, and calming the action of the heart and arteries; 4th, of increasing the metamorphosis of tissue, of modifying the chemical changes going on in the blood, and of proving active provocatives of an increased secretion of urine, whereby the elimination of the materies morbi is assisted. And if this be so, there is no difficulty in perceiving that *à priori*, soda is just as likely as potash to effect the object we have in view. That it proves so practically you have seen, in the beneficial influence which it exerted in the severe case now under consideration, and in many other cases which, for the sake of experiment, I have treated in this hospital with soda alone. But my impression, founded on practical observation no less than on theoretical considerations, is in favour of giving the mixed alkalies. We know that potash alone will cure the disease, and you have seen that soda alone will exert an equally curative power; and inasmuch as we believe that these agents operate not merely as antacids, but rather as modifiers of the chemical changes whereby the formation of the materies morbi is brought about, and as soda differs entirely from potash in its chemical properties, it is only reasonable to suppose that the one agent must exert an influence on the economy which could not be produced by the action of the other, however largely administered. If I am correct in this view, and it be admitted that potash and soda exert a modifying influence each in its own peculiar way, it follows, unless the influence exerted by the one salt interferes with that produced by the other, that the joint action of these agents must be more satisfactory in its results than the action of either of them taken separately.

It may perhaps occur to you that, if potash and soda are each capable of effecting the cure of acute rheumatism, ammonia may possess a similar power, and may possibly exert an influence more potent than either the one or the other. I have tried the question fairly in several cases, and those of you who watched the case of S. R., aged forty-eight, who was admitted into the Roseberry ward on the 18th of last October, will have formed your own conclusions on the subject. To satisfy some gentlemen as to the effect of ammonia, I gave this woman every three hours half a pint of liquid containing four ounces of the solution of acetate of ammonia, and an excess of one drachm of carbonate of ammonia. This she took regularly until the 26th, when, although the pulse had fallen from 120 to 90, the pains had not abated, and the urine continued acid. Potash was then administered, and within two days the urine was alkaline and the pains were much relieved. The truth appears to be that, although ammonia behaves as an alkali when in contact with an acid, and therefore neutralizes any acid which is present in the stomach, it does not operate as an alkali in the system, for it not only fails to render the urine alkaline, but it fails also to exert the modifying influence over the chemical changes which is necessary to get rid of the disease. Administered alone, therefore, ammonia will not suffice to arrest an attack of acute rheumatism, but it exercises a power of maintaining the fibrin of the blood in solution beyond comparison greater than that exerted by potash and soda,* and therefore I never fail to give it in conjunction with the other alkalies, except when I prescribe each alkali separately for the purpose of experiment. Whether a more satisfactory result is obtained by the joint use of these several alkalies than by the use of each them separately, it is very difficult to determine by numerical calculation, inasmuch as the inquiry demands the observation of each mode of treatment in a larger number of cases than falls to the lot of any one physician. Be that as it may, however, the more I see of this once formidable disease, the more convinced do I become that in the judicious employment of alkalies we have a complete and efficient antidote not only to the continuance of the articular symptoms, but to the occurrence of cardiac complications.

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* Dr. Richardson on the Coagulation of the Blood.

This brings me to the last point on which I mean to speak to you to-day,—namely, the existence of endocardial murmurs in the case of W. L. Systolic murmurs of functional origin are not uncommon both at the base and apex of the heart in connexion with acute rheumatism; they are much more common than is usually supposed, and I fear have often led to leeching, venesection, and other antiphlogistic and depressing treatment. Time was when a systolic murmur at the left apex was always regarded as indicative of organic mitral mischief, and even in the present day the same impression respecting it is extensively prevalent in the profession. But of its fallacy there cannot be a doubt; for although it is impossible to prove that the valves may not become inflamed and swollen, and then subsequently resume their natural condition, without the occurrence of permanent organic mischief,—nay more, although the possibility of such an occurrence must be admitted, we are bound nevertheless by the results of pathological inquiry to maintain that complete recovery under these circumstances is extremely rare. But the appearance of systolic murmurs both at the apex and base of the heart is a matter of every-day occurrence in the course of acute rheumatism, and so also is its disappearance after a few days of alkaline treatment; and the supposition that a murmur due to organic mischief will cease under treatment so frequently and so readily as you have seen it do in many of the cases admitted into the hospital under my care is repugnant to all that is known respecting organic murmurs. Thus we come back to my original statement, that functional systolic murmurs are of frequent occurrence in acute rheumatism, and are apt to mislead the unwary practitioner into the belief that inflammation of the endocardium has been set up.

You may fairly ask me in what respect these functional murmurs differ from murmurs referable to organic mischief; and, to apply the question to the case under consideration, you may ask me to point out what evidence there was which induced me to regard the murmur which existed in the case of W. L. as functional, and therefore not requiring any local or special treatment. The question may be briefly answered. The heart's action was steady and regular; there was no precordial pain or uneasiness; there was none of the peculiar anxiety of countenance which usually attends endocardial inflammation; and the murmurs, though they existed on the patient's admission into the hospital, and possibly may have been present for some days previously, were comparatively slight and of a soft blowing character, whereas if they had been due to endocardial inflammation they would probably have been louder and harsher. I do not wish you to understand by this statement that it is possible to diagnosticate between a functional and an organic murmur at a first examination, or to suppose that in the case under discussion there was anything which would have justified me in stating that the murmurs were not due to organic disease. But there was enough, as already stated, to justify the gravest doubt on the subject, and to warrant some delay before active antiphlogistic treatment was adopted. And inasmuch as alkalies are excellent remedies in endocardial inflammation, I felt no hesitation in trusting to their agency for twenty-four hours, and waiting to see what the course of the murmurs would prove. For time is a great assistant in the diagnosis between functional and organic murmurs. The murmurs due to functional causes would probably maintain pretty much the same characters after the lapse of twenty-four or forty-eight hours, or if any change were noted in them, it would probably be a diminution of their intensity; whereas if they were referable to endocardial inflammation, the mischief within that time would have made great progress, and the murmurs would probably have become harsher and more intense, and the action of the heart more or less disturbed. Practically, I believe that, provided alkalies are administered internally, little harm would accrue to a patient with commencing endocardial inflammation from the delay of a few hours before having recourse to any local or special measures for his relief; whilst, on the other hand, I am satisfied that in these cases the "nimium medici diligentia" is often productive of unnecessary suffering and undue depression, which is most earnestly to be deprecated. Therefore, although when a murmur,—especially an endocardial murmur at the apex,—arises in the course of acute rheumatism, and coincidently with its commencement pain occurs at the chest, the pulse becomes irregular and fluttering, the action of the heart turbulent, and the countenance anxious, it is impossible to resort to a special treatment too soon, or to push it too rapidly; yet, on the other hand, when an endocardial murmur exists, as in the case of W. L., unaccompanied by the symptoms above described, I would urge you to follow the course which I took in that case, and wait until the lapse of a few hours enables you to determine the character of the murmur. In a large proportion of cases the delay will satisfy you that the murmur is functional, and you will probably have the gratification of finding it subside and ultimately disappear, and of feeling that, by the caution you have displayed, you have saved your patient unnecessary suffering, or have avoided doing what must have retarded his recovery.—*Lancet, December* 20, 1862, *p.* 667.

8.—ON CASES SIMULATING RHEUMATISM.

By Dr. HENRY WILLIAM FULLER, Physician to St. George's Hospital.

[A class of cases, going by the name of rheumatism, characterised by circumscribed pain and swelling of the periosteum, is very common. The patients are always cachectic, and have often suffered from syphilis. The following is a good example of this class :]

This man had been suffering from pains in the limbs for above eighteen months, and although he positively denied having had syphilis for nearly seven years, his pains were manifestly connected with a venereal taint. He had had sore-throat and purulent discharge from the nostrils; and a large, firm, and tender node existed on the right tibia. His complexion was sallow, his tongue was clean, his bowels were regular, and his urine was acid and clear. He had not had a good night's rest for many weeks in consequence of the pain. I gave him the ordinary diet of the hospital with a pint of porter; and prescribed the following medicines: ten grains of iodide of potassium, and ten drachms of cinchona draught, three times a day. At the same time I kept applied to the painful spot a rag dipped in a lotion composed of five grains of bichloride of mercury, six drachms of compound tincture of iodine, two ounces and a half of glycerine, and five ounces of distilled water. Within ten days the pains were much relieved and the periosteal swelling was subsiding. On the 31st, being quite free from pain, he was permitted to leave the hospital.

Another instance of the same form of disease was admitted at the same time into the Queen's ward, in the person of M. A. P., a married woman, aged twenty-five. She had contracted the malady in America, and for two years had suffered severely from pains in the limbs, which medical treatment had hitherto failed to relieve. Since she was first attacked she had twice had inflammation of the eyes, and had lost the sight of the right eye, and she had also become deaf—effects which she attributed to the "rheumatism." As a child she had scarlatina, and the year before last the lower extremities were anasarcous for a period of three months. On admission to the hospital, she was cachectic in appearance, her complexion was pale, her skin cool, and tongue clean; the bowels were regular; and the urine was pale, acid, albuminous, specific gravity 1006. The catamenia had been absent four months, but she was not pregnant. Large tender nodes existed on the scalp and forehead, there was considerable effusion into the capsule of the right knee, and there was opacity of the right cornea over the pupil. The heart's action was regular, and its sounds were clear. I gave her the ordinary diet of the hospital, with a pint of porter; and prescribed the following medicine—namely ten grains of iodide of potassium, two drachms of solution of bichloride of mercury, and ten drachms of cinchona draught, every six hours. On the 17th slight ptyalism was coming on, and therefore the solution of the bichloride of mercury was omitted; the other remedies were continued, and, as she was restless at night, ten grains of Dover's powder were given at bedtime; five ounces of gin were also ordered. Gradually the pains subsided, the nodes disappeared, and she left the hospital quite free from suffering on the 9th of February, or at the expiration of four weeks from the date of her admission.

Now these two cases are fair examples of this form of disease, of the consequences to which it gives rise, and of the treatment calculated to get rid of it. Wandering pains in the limbs, occasional effusion into the capsules of the joints, inflammation of the eyes, inflammation of the periosteum—leading if neglected, to disease of the bone beneath,—are some of its ordinary effects; whilst deafness, headache, and convulsions resulting from the pressure of nodes formed on the internal table of the skull, are some of its less frequent complications. Its characters when fully developed, are so strongly marked that you can scarcely fail to recognise them; and when once recognised the treatment is exceedingly simple. The patient is cachectic, and therefore must be supported; whilst bark, iodide of potassium, and, if necessary, a little biniodide of mercury, are given, to rid the patient of his malady. Perhaps there is no form of disease in which relief is obtained more speedily or more certainly, if due care is taken to sustain the patient's strength during the administration of the remedies. But if the character of the disease be overlooked, so that proper medicines are not prescribed, or if the patient be not properly supported, no form of malady is more intractable. Baths of all kinds, alkalies, colchicum, guaiacum, suphur, and other so-called anti-rheumatic remedies, are absolutely useless. The pains not only continue unrelieved, but gradually progress from bad to worse, until structural disease of the bone has been set up, and the mischief is probably irremediable. In both the cases which I have brought before you, treatment had been had recourse to, at longer or shorter intervals, for a period of eighteen months, and the patients were worse at the expiration of that time than they were at the commencement of the attack; and yet within one month from the first adoption of appropriate treatment they were perfectly free from pain.

In both these cases, and in many others which you have noted under my charge, I have made use of the biniodide of mercury. In the first it was employed as an external application; in the second, as a remedy administered internally. But I would have you remark, that in neither instance did I prescribe the biniodide as such, but formed it extemporaneously by the addition of iodide of potassium to the bichloride of mercury. My object in so doing was to obtain the remedy in a soluble, and therefore in an active form. Biniodide of mercury, as you are doubtless aware, is a scarlet powder, insoluble in water, and like all other insoluble matters when taken into the stomach, is uncertain in its action. Much larger quantities of it must be administered than would be needed if we could make sure of its being absorbed; and whilst in some instances we fail to obtain the desired results, in others profuse salivation is induced in consequence of a large proportion of the remedy having found its way into the system. But biniodide of mercury is rendered perfectly soluble in water by an excess of iodide of potassium; and thus, whether employed externally or internally, the certainty and energy of its action are much increased: smaller doses will fulfil the object we have in view, much greater reliance can be placed upon its action, and its operation can be regulated with the greatest nicety. The contrast between its effects in the two forms is so striking and so manifestly in favour of its solution, that I should advise you to adopt this mode of administration whenever you may have need of its assistance.

Another form of so-called rheumatism which you will often be called upon to encounter is that known under the title of *gonorrhœal rheumatism*. So common is this form of disease that we are never without one or more examples of it in the wards. But although the opportunities for observation are numerous, the profession have not as yet attained to much certainty in its treatment. In private practice I am often consulted by gentlemen who for months have been taking medicine without relief; and I doubt not that you have seen many patients in this hospital who, after having taken alkalies, iodide of potassium, colchicum, guaiacum, and other anti-rheumatic remedies, and having had a succession of warm baths, hot-air baths, and sulphur-vapour baths, have left the house very little better than at the date of their admission. And is not this fact, and the frequent occurrence of inflammation of the eyes which is observed in the course of this complaint, cogent argument against its rheumatic origin? My own conviction is, that the disease is due to the action of a specific poison, and has nothing in common with rheumatism as typified by rheumatic fever, except pain in the limbs and occasional swelling of the joints. Its origin is manifestly connected with a gleety discharge from the urethra, resulting, as I believe, from some peculiar form of vaginal poison distinct from that which occasions ordinary gonorrhœa. Its course is unlike that of true rheumatism, inasmuch as when it occurs in an acute form it is seldom accompanied by much heat of skin, or furring of the tongue, or loading of the urine, is attended by excessive synovial effusion, is rarely productive of redness of the affected joints, and is never marked by inflammation of the heart; and when it presents itself in a chronic form, it is almost invariably unattended by furring of the tongue or loading of the urine, or by any of the general symptoms which characterize true rheumatism; whilst the local affection, as in the acute form, is distinguished by synovial effusion, by its stationary character, and by its obstinate resistance to ordinary treatment. The insufficiency of ordinary remedies is admitted by all, and I am sure I am enunciating the opinion of the profession when I tell you that cases of gonorrhœal rheumatism are amongst the most obstinate we have to deal with. Iodide of potassium, alkalies, guaiacum, colchicum, copaiba, and sarsaparilla, which are the medicines most highly esteemed in such cases, very generally disappoint our expectations; for although the urine be rendered alkaline, although iodism be induced, although colchicum, guaiacum, and copaiba be administered in full and repeated doses, although the patient take sarsaparilla *usque ad nauseam*, the obstinate swelling of the knees, or feet, or wrists continue, and the wandering pains in the limbs torment the patient as before. Sometimes, indeed, the pains will subside under the influence of these remedies, but the frequency with which they continue unchecked has long since satisfied me that in many instances some agent is needed which will exert a more decidedly curative power. This agent, I believe, is to be found in the biniodide of mercury. Provided only the urethral discharge be stopped, —and if it be not, measures must be taken immediately to check it,—this remedy will almost always effect the object we have in view. The cases in which I have employed this drug in the wards of the hospital are so numerous, that you have all had opportunities of watching its action; but it may be well perhaps to direct your attention to one or two cases in which its effects have been strikingly manifest.

First, let me cite the case of R. C., aged twenty-one, in Hope ward. He had been attacked with urethral discharge three weeks before admission, and for a fortnight had undergone treatment with relief, when he was suddenly seized with excessive pain and swelling of the left wrist and both knees, and with conjunctivitis of both eyes. In this state he was admitted into the hospital. His complexion was pale; skin warm not hot; tongue coated; bowels reported open, and urine clear; pulse 96; appetite good. A slight gleety discharge from the urethra still continued.

Now, here was a case in which the articular pain and swelling were severe, and in their character corresponded with what is observed in rheumatic fever, and yet the general symptoms of the case bore no sort of resemblance to those which accompany that form of disease. Not only was there no heat of skin, but the urine was clear, the appetite good, and the conjunctiva, which in true rheumatism is rarely, if ever affected, was the seat of acute inflammation. No wonder that in these cases the treatment ordinarily pursued for the cure of rheumatism proves inadequate to restrain the progress of the disease! The malady itself is of a specific nature, and requires special treatment. In the example before us, the patient was cachectic, and therefore the ordinary diet of the hospital, with a pint of porter, was given daily; a zinc injection was prescribed with the view of checking the gleet; the eyes were fomented with warm water; and the following medicine was administered three times daily—viz., solution of bichloride of mercury, two drachms; iodide of potassium, half a scruple; cinchona draught, ten drachms. This treatment was commenced on the 20th of June, and by the 30th the inflammation of the eyes had disappeared, and the swelling in the joints had greatly subsided. The medicine therefore was continued as before; the pain and swelling decreased daily; and on the 6th of July, as there was no longer any pain, the patient was permitted to leave the hospital.

The case of A. T., aged twenty-two, admitted on the 18th of June into the Fuller ward, was of the same character. This man was attacked with gonorrhœa two months before admission, and had suffered severely from pain in the limbs and joints above six weeks, and from conjunctivitis of the right eye one week; and although he had been under medical treatment, he had not received the slightest relief. His skin was natural, urine clear and tongue clean, his bowels were regular, his pulse was 96, and his appetite was indifferent. There was severe pain, but no swelling, in most of the larger joints, and the inflammation in the right eye caused so much suffering that it prevented his sleeping at night. Fish and beef-tea were ordered for his diet, a blister was applied behind the right ear, and the following draught was given three times daily—viz., iodide of potassium, six grains; solution of bichloride of mercury, one drachm and a half; nitrate of potass, one scruple; cinchona draught, ten drachms and a half. Within five days the eye was much relieved, and the appetite had improved; and as the skin still continued cool and the urine clear, I ordered him to take meat and a pint of porter for his dinner. On the 4th of July he was able to discard his eye shade; and on the 9th as he had no longer any pain in the limbs, he was permitted to leave the hospital.

These two cases are fair examples of the result of this treatment when it is commenced within a reasonable time after the accession of articular mischief; and those of you who have watched my practice in the wards are aware that I seldom have a recent case of so-called gonorrhœal rheumatism under my charge above a fortnight or three weeks. But when this disease has been improperly managed, and has run on for a period of many months, it is apt to produce thickening and stiffness of the ligamentous and other structures around the joints, especially around the ankles. This chronic thickening and pain may induce permanent disability of the joints; and as internal remedies are here of little avail, and ordinary lotions and embrocations are always of little service, I will call your attention to the only mode of treatment which I have found productive of real benefit. I will illustrate it by reference to the case of J. M'G., aged twenty-one, who was admitted into the Fuller ward on the 25th of July. This man had an attack of gonorrhœa eight months prior to his admission to the hospital, followed in the course of ten days by pain and swelling of several of the larger joints and especially of the feet and ankles. He underwent treatment with partial relief, but had never got rid of the pain and swelling of the ankles, although blisters had been applied and a variety of lotions and embrocations had been employed. At the date of his admission his ankles were both swollen, the structures external to the joint being much thickened; they were not tender on pressure, but were so painful on motion that he could scarcely walk across the ward even with the aid of two sticks. His general health was good, his functions were properly performed, and he had no pain in any other limb or joint. He had a thick discharge from the urethra, which had made its appearance from time to time ever since the attack of gonorrhœa.

Now this case is a type of many which will come before you in practice, and I would have you look closely into its history. What think you were the indications for treatment? The urethral discharge was an abnormal feature requiring local treatment to repress it, and a zinc injection which was ordered for that purpose effected its object in less than a fortnight. But what indications were there for general treatment,—for the administration of medicine? Assuredly none. This man's health was good, his appetite excellent, his functions were regularly performed, his secretions were natural, his pulse was quiet, and there were no wandering pains in the limbs or other evidences of a poisoned condition of the blood. Accordingly I abstained from the administration of medicine internally, and addressed myself to the relief of his feet and ankles by external means. I gave directions that a cold douche should be applied to these parts daily for the space of ten minutes, or until he could bear the shock of the water no longer, and that then, without previously wiping or drying the feet, active friction should be made use of until warmth had returned to them. This active heavy rubbing was continued for about twenty minutes on each foot and ankle daily. When it was concluded, and the circulation in the part was throughly re-established, the feet and ankles were enveloped in lint steeped in the following lotion: namely, compound tincture of iodine, half an ounce; glycerine, two ounces and a half; water three ounces. The lint was then covered with flannel, and was kept on the part until the following morning, when the douche and friction were made use of as before, and then the lotion was reapplied. The result, as you have seen in many other cases, was that within a few days he was enabled to discard his sticks, and he left the hospital on the 22nd of August free from pain and walking comfortably. In many instances of the same kind, in which patients have been admitted with the strictures around the knees thickened and with chronic effusion into the capsules of the joints, I have often employed the same treatment successfully, the only variation being the use of a bandage, firmly applied over the lotion-steeped lint, instead of a simple piece of flannel—the support which the bandage gives in these cases being favourable to absorption of the synovial fluid.

Before quitting this subject, I would make a few remarks respecting this method of treating chronic thickening of the joints. It is not the mode of treatment usually employed, but it is eminently successful, as those of you who have watched my practice can testify. Case after case has come before you in the wards in which persons who have been suffering many weeks, or even months, from thickening and stiffness of some of the larger joints, and thereby have been incapacitated for work, have recovered the use of their limbs, and have left the hospital after a few weeks of this treatment. This, too, has happened in the instance of many patients, who, prior to admission, have had their joints blistered, painted with iodine, and otherwise treated without relief. In truth, it has always appeared to me that the ordinary mode of employing iodine in these cases is wrong in principle, and often useless in practice. The compound tincture of iodine is the preparation commonly employed, and it is painted over the affected part under the idea that it will act as an absorbent. But when used in this manner, it dries in a few minutes, and destroys the cuticle after two or three applications. In some instances, it may be useful as an escharotic; and, in others, it may prove serviceable as a mild counter-irritant; but it is repugnant to common sense to suppose that when applied in this manner it can exert a distinctly absorbent influence. Further, by destroying the cuticle, it soon creates a bar against its own employment, and utterly precludes the use of friction and other measures which may be deemed likely to be productive of relief. The method which I adopt has no such drawbacks. It does not lead to destruction of the cuticle; it does not even produce cutaneous irritation, unless the lint be covered with oiled silk or gutta-percha, in which case a crop of pustules will make their appearance within forty-eight hours; it ensures a constant absorption of iodine, and a constant stimulus therefore to the removal of the effused fluid; it admits of the application of the douche and friction—two of our most potent auxiliaries for stimulating the local circulation and getting rid of chronic thickening around the joints; and it effects the object in view more rapidly and more certainly than any plan of treatment with which I am acquainted. The formula which was employed in this case contains as much iodine as the skin will bear without suffering, and the glycerine in the lotion prevents the drying of the lint, and does away with the necessity for wetting it more frequently than once in twenty-four hours. The patient is thus saved unnecessary trouble, and the pain and irritation resulting from the destruction of the cuticle are avoided.—*Lancet, January* 31, 1863, *p.* 111.

9.—ON THE TREATMENT OF ASCITES AND ANASARCA.

By Dr. J. Birkbeck Nevins, Liverpool.

[In a paper read before the Lancashire and Cheshire Branch of the British Medical Association, Dr. Nevins observes:]

The principle of treatment in these diseases which I wish to lay before you is, the importance of *operating early for the removal of the fluid by means of tapping or by acupuncture, instead of looking upon this method of treatment as the last plan we should resort to.* I believe that, when the mechanical removal of the fluid is effected at an early period, the chances of the patient are much improved; and that, instead of regarding tapping merely as a palliative to relieve the last urgent sufferings of the sick person, we ought to look upon it as an important curative agent. As I am not aware that this course has been prominently laid before the profession, I am desirous of stating the grounds upon which I recommend it, though it is very possible that the same reasons and the same treatment may have been suggested by other members of the profession, and have escaped my notice.

I have frequently observed the difficulty experienced in obtaining the free action of diuretics in cases of anasarca, until the fluid has been removed by some other means, after which the diuretics have produced the desired increase of urinary secretion; and, as a consequence of this, the further accumulation of fluid in the cellular tissue has been prevented. The means by which the fluid has been removed in the first instance have sometimes been copious sweating in a steam-bath, sometimes free purging by hydragogues, and at other times acupuncture of the legs and feet; but, in whatever way the object has been accomplished, the effect has been the same,—viz., the natural action of diuretics, which had previously failed to operate.

In looking for an explanation of this phenomenon, I have come to the conclusion that the secreting portions of the kidneys are so enfeebled in their action by the pressure of the accumulated fluid present throughout the cellular tissue of the body, that they are unable to perform their duties aright, until the the pressure is removed by some of the various methods just mentioned. The steam-bath or hydragogues would naturally be tried in the first instance; but, if they *do not speedily produce the desired effect, I urge the early employment of acupuncture*, and think the results of my experience are now sufficiently extensive, to warrant my bringing the subject thus prominently before my medical brethren. I have for some years laid this principle before my class in lecturing on diuretics at the Medical School; but I have waited for accumulated experience before making more public mention of it.

In addition to its effect in promoting the action of diuretics, I consider the early employment of acupuncture important with reference to the condition of the patient's skin. When it is employed early, and before the skin has become enfeebled by the great and long continued distension, I have never seen any bad consequences follow the operation; but when it has been delayed to the last moment, and the legs have long been swollen to a painful extent, I have seen inflammation follow the operation, which has been troublesome in more than one instance.

In performing the operation, I advise that a few punctures only should be made,—say half a dozen in the first instance,—in one leg and foot; and that the patient should afterwards sit up as much as he conveniently can. The limb should be folded in flannels, which should be frequently changed as they become wet; and it is frequently necessary to apply warmth to the foot in bed. I recommend the employment of a common *stout* round sewing needle, in preference to a cutting edged needle, which I have for some time ceased to employ, as I have seen the wounds made by a three-cornered needle followed by inflammation more frequently than those made by a round one. The round punctures do not allow the escape of the fluid quite so freely as the others; but the comparative freedom from subsequent inflammation is important; and the operation with a common needle supplied by the patient himself, appears much less formidable to him than when the instrument is taken from the surgeon's instrument-case. As soon as the oozing of fluid ceases, the operation should be repeated if the swelling still continue.

With reference to the treatment of *ascites by early tapping* I have less extensive experience; but the following cases appear to me to hold out sufficient encouragement to warrant the fair trial of any plan which gives us reasonably increased hopes of cure in so generally hopeless a disease as dropsy of the abdomen. When the ascites is apparently dependent upon simple disease of the serous membrane (as, for example, in hydrocele), I think we have considerable encouragement in hoping for recovery; and even if it is due to cardiac obstruction, or to disease of the liver, I should still adopt the same mode of treatment, because it offers tangible advantages to the patient, with no corresponding disadvantage, so far as my experience extends.

It is familiar to us all how difficult it is to obtain the good effects of elaterium, in consequence of the vomiting which it excites; and, owing to the circumstance, we are frequently obliged to resort to tapping merely as a palliative, to relieve the urgent sufferings of the patient. Now, if the delay of the operation enabled us to make one tapping suffice for cure, we should be warranted in urging our patient to bear his sufferings, in the expectation of radical relief; and he would be repaid for his patience. But, since we find that repeated tappings are the rule, and not the exception, it appeared to me very doubtful whether, after all, the benefits from delay did repay the prolonged distress from waiting; and I, therefore, repeated the tapping as soon as the patient became uneasy, without waiting for positive suffering.

I have further observed that, in some cases, hydragogues acted upon the bowels after the patient had been tapped, although they had produced vomiting, or failed to purge before the operation; and I venture to suggest, therefore, that in this case also, the removal of pressure enables the organs to perform their functions, which they were previously unable to do.

I have now adopted this plan of treatment in three cases of ascites, which have resulted in perfect cures. The first case was that of a woman in whom the early tappings were delayed in accordance with our usual practice, and she was tapped many times before the fluid ceased to accumulate; but at length she was discharged cured, and died eighteen months afterwards of some other disease. The second patient was a man, who recovered after three tappings; and the third was a woman, who also recovered after three tappings.

I regard *constant mechanical support of the abdominal walls as a very important part of the subsequent treatment*, by enabling the tissues to regain their natural tone and vigour, after the extreme and enfeebling distension to which they have been so long subjected; and I, therefore, strictly enjoin the constant use of a flannel abdominal bandage, which I do not allow to be given up until the fluid has ceased to reappear.

In the course of *one or two days after the operation, I always resume the use of elaterium or jalap* in such doses as the stomach will bear without sickness, which ought always to be carefully avoided; and I have frequently found that a single pill daily, containing one-sixteenth of a grain of elaterium, produces copious watery purging without sickness after the operation, though the patient derived no benefit from it before the tapping.

The health should be sustained by a liberal diet, of which malt liquor or spirits may form a part or not, according to the circumstances of the case. Iron tonics have also appeared to be useful.

I feel that some apology is necessary for laying so much stress upon the results of so small a number of cases as the three which I have brought before you; but the principle upon which the treatment is based appears to me to be rational; and, if we can throw even but a little hope into the treatment of a disease which is generally so intractable as ascites, our patients cannot be great losers, and may possibly be great gainers. I trust that the kindness of my medical brethren will induce them to communicate to the profession whether their experience in future bears out or contradicts the principles now laid down; viz., that, in the treatment of ascites and anasarca, we ought *to resort early to the mechanical removal of the fluid*, instead of considering tapping as a last resource; *to employ long-continued mechanical support afterwards; and to administer such doses of appropriate medicines* as the patient's stomach will bear, *early after the performance of the operation.—Brit. Med. Journal, Feb.* 7, 1863, *p.* 139.

DISEASES OF THE NERVOUS SYSTEM.

10.—ON THE TREATMENT OF EPILEPSY BY BELLADONNA.

By Dr. J. S. Ramskill, Assistant Physician to the London Hospital, and Physician to the Hospital for Epilepsy and Paralysis.

Concerning the treatment by, and action of, belladonna in epilepsy, I will give you, in a short compass, the results of my experience in its use. First, you must not always, nor even usually, look for immediate and palpable beneficial results. The number of fits at first may not lessen in equal times; very frequently, the reverse obtains; and you may expect, for three or four weeks after commencing it, even in the most appropriate cases, a complaint that the patient gets worse; but, after six or eight weeks, if any amelioration occur, it will be decided and progressive. At first the dose should be very small, and gradually augmented until the pupil shows signs of its action, and the patient complains of both alteration in sight and dryness of throat. Having obtained this result, and maintained it for some weeks, the dose may be gradually diminished; but its effects on the eye and throat are not to be so diminished as to become imperceptible to the patient, but only so far lessened as to cease causing absolute discomfort. The other toxic effects of belladonna are wholly uncalled for. Patients vary greatly, both as to susceptibility in the action of the drug, and in other respects. The annoyance as to dry throat and disturbed vision, which, at the expiration of a month, may be said to be unendurable, will now and then cease, the dose being the same, or even slightly increased; but I may remark, these cases always improve most rapidly. I prefer to give the drug in an eighth of a grain dose three times, or only twice, daily, for a week; then a quarter of a grain for fourteen days; a third for the next fourteen days, at which time its physiological action will in most cases be manifest. I think it wise to halt at this dose for two months or three months, slightly increasing the dose if the patient shows diminished susceptibility to its influence, decreasing it if the reverse happens, and then gradually dropping it to the quantity first administered. I have given as much as four grains for a dose, but very rarely. I think it imperative to say, that I have never been able to give in epilepsy the large doses which Dr. Fuller has succeeded in administering in other diseases of a convulsive character. In this remark I am supported by the authority of my colleague, Dr. Brown-Séquard, who has arrived at the same conclusion. One objection to the use of belladonna, when you cannot see your patient at regular intervals, arises from its uncertainty of strength and corresponding difference of action. To those who wish to use a preparation of uniform strength, having similar and, in some cases, improved properties of belladonna, the salts of atropia are now easily procurable. The best of these is the valerianate of atropia; the commencing dose a hundred-and-twentieth of a grain. Hitherto, I have preferred belladonna, having had a strong desire to find what it could, and, if possible, what it could not, accomplish in the treatment of epilepsy. It is right to say there are different methods of administering belladonna. Trousseau gives a centigramme of the extract and an equal quantity of the powder of belladonna for the first month, in the evening of each day. He gives it at this time because of the frequent nocturnal character of epilepsy, and partly because of the disagreeable effect on the sight and throat during its early administration. During the second month he gives two such pills at the same time, and during the third month three pills. If, at the end of six or nine months, the frequency of the fits is decreased, he increases the dose. He asserts that, of 120 patients, he has cured twenty. A most important question now arises—Do we know anything of the nature of the action of belladonna beyond the empirical results obtained in treatment? If a drop of solution of belladonna or atropine be dropped on the foot of a frog properly prepared, and fixed on the field of a microscope, the blood-vessels will be seen to contract, and they will remain in this condition for a considerable time. For comparing the action of opium, a solution of the latter, similarly prepared, was applied to another part, and the vessels were immediately dilated. Now, belladonna, internally administered in medicinal doses, causes, first, dilatation of pupil, with dimness of vision; secondly dryness of throat and difficulty of swallowing; thirdly, increased tone of involuntary muscle; fourthly, it relaxes the bowels, and cures incontinence of urine, arising from weak sphincter vesicæ. As dilatation of pupil is one of the earliest phenomena, let us see if we can account for it. There are two sets of fibres in the iris. It is well known that the sympathetic is the motor nerve of the external longitudinal fibres of the iris, which radiate from the centre to the circumference. The branch of nerves supplying these fibres comes from the cervical ganglia of the sympathetic. Excitation of this nerve, from any cause, will cause a contraction of these longitubinal fibres, and a corresponding dilatation of pupil. There is also a circular set of fibres immediately surrounding the margin of the pupil. This set is under cerebral control; that is to say, its motor supply comes from a branch of the third nerve. Any irritation in the brain, or along the trunk of the nerve, or an excitation by light on the retina acting in a reflex manner, will stimulate this branch of the third to action, and cause contraction of pupil.

But we may have dilatation of pupil without increased action of the sympathetic; it may be acting normally, then the third nerve must be supposed deficient in power. This is a common result observed in compression of brain. On the other hand, contraction of pupil may be present without abnormal activity of the third being necessarily supposed. This condition is invariably produced by section of the sympathetic in the neck. Dilatation of pupil may, in short, depend on the action of the sympathetic being in excess, or in diminished power of the cerebral nerve. In epilepsy it is easy to observe, from collateral symptoms and the general condition of the patient that dilated pupil, when it exists, which is much rarer than a normal condition, is usually caused by an active sympathetic overpowering the third nerve. The same dilatation may be observed in most convalescents after acute disease, and in most affections involving extreme debility; but here it would be more correct to say that the dilatation was rather the effect of a depressed condition of the third cerebral nerve accompanying a normal sympathetic, than of an active sympathetic accompanying a normal condition of the cerebral nerve. I have said the branches of the sympathetic nerve which go to the iris come from the cervical sympathetic. Dr. A. Waller, with Professor Budge, have made experiments, which seem to prove that the nerve fibres of the cervical sympathetic, which go to the iris, originate from the spinal cord between the sixth cervical and the fourth dorsal vertebræ. Dr. Brown Séquard has ascertained that the origins of the fibres of the sympathetic going to the iris are still more extended. I have mentioned that division of the cervical sympathetic allows the uncontrolled third cerebral nerve to contract the iris. Dr. Brown-Séquard has shown that a section of the spinal cord, as high as the level of the fifth cervical, or as low as the ninth or tenth dorsal vertebræ, affects the iris in the same manner, but in a less degree than section of the sympathetic. On the other hand Schiff has shown that some of the fibres animating the iris ascend the cervical part of the spinal cord, and most probably go up to the medulla. I may also say here that the sympathetic is the motor nerve of the blood-vessels, supplying various parts of the head. It is especially interesting to know the origin of these vaso-motor nerves, especially in relation to loss of consciousness, the initial movement of a fit of epilepsy, and also in regard to the pathology of the *petit-mal*, as well as the great light such knowledge would throw on the action of belladonna in epilepsy. Dr. Brown-Séquard discovered some years ago that the motor nerves of the blood-vessels going to various parts of the head, come out chiefly from the spinal cord by the roots of the last cervical and first and second dorsal nerves. He thinks, however, their real place of origin to be partly the spinal cord, partly the higher portions of the encephalon, but chiefly the medulla oblongata and the neighbouring parts of the encephalon. In the case of R. P. it will be remembered the ferrum candens was applied to each side of the spine, opposite the last cervical and first dorsal vertebræ. The reason will now be apparent. The vaso-motor nerve fibres are able to contract the blood-vessels directly, when excited. We hope, by frequently cauterising the tissues opposite the seat of exit of these nerves from the spine, to effect some change in the nutrition of the parts to which these nerves are distributed. We can now understand the nature of the action of belladonna in producing dilatation of the pupil; and from its effect on the iris we can deduce a strong probability of the nature of its action in epilepsy. It is a stimulant to the sympathetic, the motor nerve of the blood-vessels, and it is only on this supposition we can account for the other physiological effects of the drug.

I would add, although experience shows belladonna is one of the most powerful contractors of the blood-vessels of the spinal cord and its membranes, it has a comparatively feeble action on those of the brain. I speak of its administration in medicine—not in poisonous or fatal doses. Hence arises its extraordinary adaptability in epilepsy, where we have dilatations of vessels or turgescence in the medulla and its neighbourhood: of its still more marked efficacy in inflammation, and congestion of the spinal cord and its membranes; as well as of its comparative inutility (administered alone) in those cases of morbid activity of brain, connected, as we think, with more or less congestion of grey matter, in some forms of incipient insanity, associated with sleeplessness and suicidal tendency, as well as in some other cerebral diseases.—*Med. Times and Gazette, Nov.* 22, 1862, *p.* 540.

11.—ON THE TREATMENT OF TRAUMATIC TETANUS BY NICOTINE.

By Jolliffe Tufnell, Esq., Surgeon to the City of Dublin Hospital, and Examiner in Surgery R.C.S.I.

[The following interesting case of tetanus is one following upon compound fracture of the right forearm.]

J. D., æt. 42, a tall, muscular man, a sewer maker, of very intemperate habits, was brought to the City of Dublin Hospital on the evening of the 28th of February, 1862, suffering from a severe compound fracture of the right radius, three inches above the wrist. The accident had occurred about twenty minutes before admission, and was occasioned by the falling of a heavy flagstone upon the forearm. There was a good deal of laceration of the soft parts, and considerable hemorrhage. The wound, which was three inches long and triangular in shape, had its base to the ulna, and was filled with blood and clay; the upper end of the lower fragment of the radius was thrown outwards upon the ulna; the hand was pronated and curved inwards.

The wound having been carefully cleansed, the fracture was adjusted, and the arm placed upon a pistol splint; water-dressing with flannel-tailed bandage surrounding all. Wine and an opiate was given. Everything went on well until the evening of the 12th of March, when the patient said that he thought he must have caught cold, as he had pain and soreness about the jaws, with difficulty of mastication. He had also a peculiar tendency to sigh. Shortly afterwards he complained of pain in the left mammary region, and the abdominal muscles, when examined, were found somewhat tense. There was at this time no rigidity of the forehead, eyelids, or mouth. The pulse was soft, regular, and natural at 68, the respiration normal, and the bowels had been acting regularly. He was ordered, however, a bolus of five grains of calomel with one drachm of compound powder of jalap.

On the following day (13th), the facial muscles exhibited the characteristic contractions of tetanus. The trismus was now determined, and the cervical muscles, especially upon the right side (that of the accident), engaged. The right rectus abdominis was more tense than the left. The bowels had been freely acted on by the aperient, and the discharges were not dark.

He was now ordered to take one drachm of chloroform agitated with an ounce and a half of linseed tea every fourth hour, and a bag of ice directed to be applied to the cervical spine. The wound was looking healthy, and beginning to cicatrize at the edges.

Upon the 14th there was an advance in the symptoms which seemed to be kept partially at bay only by the chloroform. The muscles of the neck and abdomen had become exceedingly tense, and there was pain through the attachments of the diaphragm. The muscles of the extremities, too, were now attacked by clonic spasms. The pulse had increased. Chloroform was continued.

Upon the 15th he was worse. The masseter muscles and sterno-mastoids were very rigid, and the respiration was rendered difficult by the viscid mucus which accumulated in the bronchi, and could not be got rid of by expectoration. (He had previously suffered frequently from bronchitis.) He could swallow liquids, but with great difficulty. As the bowels had not acted since the 13th, a purgative enema was ordered, and the chloroform directed to be given more frequently; every third or second hour as required.

On the 16th the symptoms were unaltered, but the pulse was becoming frequent, rising to 90 during the paroxysms of pain. The chloroform having been administered for three days, and the case not beginning to improve, it was determined to employ nicotine, and the Rev. Professor Haughton having kindly brought some of that prepared under his own direction, the strength of a single drop of which is equivalent to 23·2-10th grains of Virginian Cavendish tobacco, the first dose was administered at eight p.m.; one drop being dissolved in a dessertspoonful of white wine mixed with an equal quantity of water. The pulse, prior to the nicotine being given, was 72; it speedily rose to 84, and within *forty seconds* of the medicine being swallowed, the whole surface of the body was bedewed with sweat. The welling up of the perspirable fluid through the pores of the skin was remarkable, it being absolutely perceptible; these drops of fluid being so large that they might easily be flicked with the hand from off the forehead. The spasms almost immediately relaxed, the corrugation of the brow and frightened expression of the countenance disappearing. This improvement was, however, only transient, and the nicotine was repeated every two hours up to two a.m., when, in consequence of the unpleasant taste and nauseating effects, the patient refused to take any more. At six a.m., he consented, however, and continued to do so regularly every two hours, the dose being increased to two drops, or nearly a drachm of Cavendish tobacco. The stools brought away by the turpentine injection were for the first time dark, green, and fœtid. From the commencement of the attack a most liberal allowance of nourishment, in the shape of essence of beef, jelly, and wine, had been allowed, which was still continued, although swallowed with great difficulty, and got down between the wedges which separated the molar teeth. Fomentations of a strong decoction of tobacco were applied over the abdomen, and with apparently some good effect; a liniment of croton-oil was also rubbed over the surface; and the wounded arm enveloped in a soft anodyne linseed poultice, made with a drachm of the extract of belladonna dissolved in a pint of boiling water.

18th. Seven days had passed over since the first symptoms had exhibited themselves, and two since the nicotine had been employed. All matters were now aggravated. The patient had become ungovernable, and would take no medicine by the mouth, and fluid of any kind he could hardly swallow. The spasms were increased in intensity, of opisthonotic character, and recurring at intervals of eight minutes. The breathing was very quick and laboured, being 44 per minute, and the face congested from mucus, which was kept churning up and down in the trachea; the pulse 130 in the minute; the surface of the body wet and cold. As it was impossible to give the medicine by the mouth, a couple of drops of nicotine were added to a glass of wine-and-water and injected into the rectum and held. The spasms relaxed under its influence, the power of expectorating returned, and the heart's action became regular again. Towards the afternoon delirium set in, and the patient now acquired full voluntary power over the muscles of the arms and legs. He endeavoured to get out of bed, and struck wildly at those around him, and was so violent that it was necessary to put him under the influence of chloroform by inhalation, which was soon accomplished, to the great relief of the patient, who afterwards rested quietly for some time. He was conscious on awaking, and asked for water to be given him to drink. He was, however, so much exhausted that two ounces of whisky, with an equal quantity of warm water, were thrown up the rectum and retained, after which he took some strong beef-tea by the mouth. During the violence of the patient, the fractured radius became bent almost to a right angle with the ulna, and the deformity of the arm was consequently extreme. An attempt was now made to remedy this deformity, but such aggravation of the symptoms was immediately produced that it was necessary to discontinue the effort, and the arm allowed to assume the position the patient chose, a scruple of watery extract of opium dissolved in four ounces of water being first applied (by means of lint saturated with this lotion) to the wound. The bowels being now confined, an enema of turpentine, assafœtida, and castor-oil was ordered to be administered, which secured the discharge of some dark-green fœtid stools.

19th. The nicotine in two-drop doses was now injected by the rectum regularly every second hour, the patient being supported by wine, beef-tea, and essence of meat, introduced through the opening left between the wedges placed between the molar teeth. Tobacco fomentations were applied to the abdomen of the strength of a drachm to the pint of boiling water.

20th. Patient complained to-day of great pain in the biceps muscle of the affected arm, and also under the scapula of that side. Suffocative catarrh was also present. A mustard plaster enveloping the whole chest was ordered to be applied, and the whisky enemata repeated, the nicotine being continued in injection every two hours. The discharge from the wound was now purely sanguineous, and all attempt at cicatrization had for some while ceased. The retraction of the anus was now so great as to render the introduction of the enema-tube an act of much difficulty.

22nd. The fifty-sixth drop and the last dose of nicotine administered to-day, the countenance being placid, and no spasm having occurred for some hours. There was great aching pain in the lumbar region and soreness of the muscles generally, especially on the right side of the body. The urine passed to-day contained a highly lateritious deposit, but did not possess the smell of nicotine; patient very weak. He was now ordered an injection consisting of five grains of quinine, the yolk of an egg, an ounce of whisky, and three ounces of milk to be administered every sixth hour.

23th. The power to open the mouth to some slight extent had returned. The discharge from the wound showed signs of purulent secretion.

24th. Patient not so well; pulse varying in force and rapidity from 104 to 140; he had pain in the diaphragm and occasional spasms; the discharges from the bowels brought away by an oil draught given overnight were black and offensive. The aperient was repeated by the mouth with a full dose of opium shortly after.

26th. Better, though very weak.

28th. Slept well; was free from pain and spasm, and the wound looking florid and healing in. The quinine injection repeated night and morning.

31st. Considerable soreness of the abdomen and diaphragmatic region being complained of, a large belladonna plaster was directed to be applied over it.

From this date the recovery was gradual and progressive, and he was discharged from hospital in good general health, and with partial use of the injured arm, on the 10th of May.

I have abbreviated this case (as far as I well could) so as not to omit the leading features and facts. That the man owes his life mainly to the nicotine, I think must be fairly assumed, for under the use of chloroform he was gradually getting worse, and the spasms not coming under control. It is true that for a considerable period after commencing the nicotine treatment the disease progressed in intensity; but still a decided impression and relaxation of spasm followed upon each separate employment of the remedy. To chloroform, however, I was greatly indebted for the power which it gave me in controlling the violence of the delirium, and obtaining for the patient, and his attendants, rest.

The advocacy of the treatment of tetanus by pure tobacco is, I believe, due to Mr. Royston, at least it is to him that Dr. O'Beirne alludes in his paper on the subject published in the third volume of the "Dublin Hospital Reports." The introduction of nicotine, the active principle of this plant, to Rev. Professor Haughton of T.C.D. To him I believe we may fairly accord the merit of its introduction into recognized practice. He has already, in the Dublin Quarterly Journal for August, 1862, published two or three cases in which this line of treatment has been resorted to, and in which its powers have been manifestly shown—cases which I would strongly advise every one interested in this subject to read.—*Dublin Medical Press, Jan.* 7, 1863, *p.* 1.

12.—TREATMENT OF CASES OF TETANUS.

By Dr. C. Fleming, Surgeon to the Richmond Hospital, Dublin.

[After the reading of the preceding paper by Mr. Tufnell, before the Surgical Society of Ireland, Dr. Fleming brought before the Society the following cases which had occurred under his care at the Richmond Hospital. The first was one of "frightfully complicated fracture of the leg." On the thirteenth day after the accident the man complained of pain and uneasiness about the muscles of the jaw.]

From the nature of the fracture he was labouring under, the man was using at the time, as was natural to expect, stimulants. These were continued, and he was put upon large doses of opium. In three or four hours after the supervention of the symptoms already mentioned—namely, uneasiness about the muscles of the jaw and difficulty in deglutition with sense of sore throat—the man's jaws got completely locked, and his abdominal muscles became as rigid as a board. As soon as he could he procured the advantage of the gentleman whose name had been so justly mentioned that evening with an approbation which they all must sanction—he meant Prof. Haughton—to see the case with him. He saw at once that the case was one likely to be rapid in progress, and he thought that to test the value of nicotine would be to test it upon that case, and to the exclusion of any other remedy at the time. Nicotine being a London preparation was not easily had in Dublin ; nevertheless, he was able to procure some from Bewley and Evans. Prof. Haughton accompanied him to the hospital. When they reached it the man had a most frightful paroxysm. He need not describe the appearances, for they were familiar to all. They gave him at once two drops of nicotine, adding a little spirit and a small quantity of water. He swallowed it, and on the mixture reaching his stomach he was seized with a partial attempt at a paroxysm. At the same time he exhibited signs as if his stomach was about to discharge, and an enormous quantity of saliva escaped from his mouth. Previous to this the smallest knife could hardly have been passed between his teeth, and unquestionably the instantaneous effect of the nicotine was to enable a separation, to a certain extent, of the teeth, and to enable him to swallow with greater ease than he had previously done. They remained at the hospital about an hour to see the result, and they particularly noted the condition of the muscles which had been at first most rigid. That rigidity materially diminished, and the sterno-mastoid muscles could be rolled through the fingers with a sort of movement. The nicotine was continued in doses of two drops every third or fourth hour, but certainly was very often refused by the man from the very nauseous and disagreeable odour it emits; however, within twelve or fourteen hours he took eight drops. Although the disease appeared to be checked, so far as regards the severity and frequency of the paroxysms, which were diminished, still it advanced rapidly, and the man died at the period when they usually die—namely, the third or fourth day after the supervention of the tetanic symptoms. This was a rapid and an acute case. In treating it he also gave what was admitted by all to be occasionally very beneficial—namely, tincture of the resin of Indian hemp.

Shortly after, a little boy, aged about twelve years, presented himself at the hospital with all the characteristic symptoms of tetanus marked upon his countenance. He had a large semicircular scalp wound on the left side of the frontal region of the head and passing across. About a month had intervened since the occurrence of the wound, and gradually, as the boy stated, he experienced a difficulty in separating his jaws in mastication. At the time of his admission into the hospital it was totally impossible to pass anything between his teeth, so locked were his jaws. He was unable without difficulty to swallow fluid nourishment, but he could not take nourishment in any other but a fluid form. He was doubtful at first whether the case was tetanus or trismus: however while the pupils of the hospital were trying to get the patient to take a drink, he gave a frightful scream and experienced a fearful paroxysm of tetanus. In this case the disease supervened at the later period mentioned. He put the boy under a treatment which had often been considered very successful by the late Professor Colles—namely, calomel and opium, and he was rapidly salivated. He gave him no nicotine, but limited himself entirely to stimulants of all kinds, supporting him by wine and such means, and he put him under the influence of opium. Between the calomel and the opium he gradually but perfectly recovered, his jaws separated slowly, and he left the hospital entirely well at the expiration of four or five weeks from the date of his admission. —*Dublin Medical Press, January* 7, 1853, *p.* 3.

13.—ON THE EMPLOYMENT OF ELECTRICITY IN HYDROPHOBIA AND TETANUS.

By Dr. H. Lassing, New York.

Hydrophobia.—In October, 1859, I was called to see a young man, aged 19, who, as the neighbours said, had been bitten by a mad dog some two months previously. The dog had refused food for over a week before he bit this man. Afterwards he foamed at the mouth, appeared to be blind, and running in his mad career against a stone wall, dashed out his brains. This young man had been sick several days, complaining of pain over his body, headache, &c., and when I saw him, had strong symptoms of hydrophobia, such as extreme nervousness, spasms, dyspnœa, great thirst, foaming at the mouth, extreme anxiety, pain and uneasiness about the præcordia. He was continually begging for a drink of water, but when any was brought evinced great horror of it, and when exposed to his view for a few minutes he was thrown into convulsions by the sight. I began by exhibiting opium and morphia, but after administering five grains of the former, and one grain of the latter, and finding no benefit therefrom, resort was had to various narcotics and sedatives by the mouth, anus, epidermis, and even inserted into the cellular tissues and glands, without effect. Various other remedies were administered, all without effect, when I asked the privilege of several professional brethren present, to try electro-magnetism, which was cheerfully accorded me.

The patient by this time having become entirely unmanageable, making attempts to bite those around him, he was secured and bound to a mattress; a copper wire being bound around both feet, the conductor of the negative pole was attached to this wire, and the conductor of the positive pole through a sponge saturated with vinegar and salt was applied to the throat, and over the spine and body generally, with the full power of the battery. This produced instantaneous cessation of the spasms, and while under the influence of electricity, the patient willingly drank liquids, and was free from the usual horror of them; but if they were near him, and the current of the machine was interrupted, all the previous symptoms would recur. I applied the current one half hour, the patient then seemed easier, but the symptoms of hydrophobia, though slighter, were still present. At intervals of an hour I applied the current again for half an hour, and after twelve hours from the time I first applied it, the patient seemed suddenly to collapse; the countenance seemed more anxious; the pulse, which was previously quick and full, now became very feeble; the tongue, previously swollen and dark red, seemed more natural; and the eyes, which were glaring, and pupils dilated, now appeared glassy and sunken. The general appearance of the patient can be better described by saying that he looked like a sea-sick person just previous to emesis; when suddenly a copious perspiration was thrown out, and free vomiting and purging followed, and he finally fell asleep. He awoke in about two hours, and appeared more normal, complaining only of a severe headache, and great weakness. We then gave him an infusion of scutellaria lateriflora, and left him. About a week subsequently, being sent for, found the patient had had a slight spasm again that day, which, however, never returned. The current, which he seemed to dread as much as fluids, was once more applied, and the patient left asleep. No more symptoms of hydrophobia recurred, and the patient did well for several months, when I lost sight of him. I should say that no signs of hydrophobia recurred, had I not read a few weeks since, in an account of a battle, that he was among the killed while fighting in the rebel ranks.

I omitted to state that chloroform and ether were both tried without any effect, previous to the use of electro-magnetism.

Traumatic Tetanus.—A little girl, aged nine, had run a needle into the sole of the foot, and while endeavouring to extract it, broke it off. The piece remaining worked its way through the integuments, and, as I am informed, could be felt upon pressure at the instep, when a surgeon was called in, and after a severe operation of over an hour, partially performed while the patient was under the influence of ether, broke off another piece, not, however, without severely contusing the integuments and muscles of the part. During the ensuing night spasms set in, which, I now think, can be ascribed to the hemorrhage and nervous irritation; but these soon ceased, and the foot and limb became terribly inflamed. On the night of the ninth day, the spasms recurred, and trismus set in. I was then called in, and ordered the tinct. opii, which my predecessor ordered as a lotion, to be stopped; considerable fluctuation could be felt, but the foot felt very hard, and appeared red, and highly inflamed. Patient's countenance betokened great anxiety and pain; pulse irregular. Partially owing to the spasms of the abdominal muscles, and partially to the large quantities of opium previously administered, she suffered from obstinate constipation. I immediately injected about a pint of a solution of ox gall in water, and retained it by pressure, but no stool was produced; I then extracted two lateral incisors, and inserted the negative pole conductors, properly insulated on the tongue, and passed the positive pole up the rectum, which produced instantaneous peristaltic movements, and was followed, in about fifteen minutes, by a copious stool. While the current was passing, the spasms ceased, the patient was able to open and shut her mouth. After applying the current for a few minutes over the body generally, I connected the negative pole with a curved bistoury, and laid the tumefied parts well open. A large quantity of pus escaped, and with it the remaining piece of the needle. I then dressed the wound with simple cerate, and administered morphia in syrup. papaveris, in 1·32 grain doses. Next day, after a good night's rest, found the patient much improved; pulse more regular; free perspiration had set in, and the wound seemed healthy, discharging considerably. A short spasm returned this day, but was speedily subdued by the electro-magnetic current, which was mildly applied over the body generally, especially the spine and affected limb. Every day for a week, beef-tea, broth, brandy-punch, and toast were freely given. The wound healed by first intention, and the patient was out of her bed in another week; and now, two months since I treated her, is playing around the street, perfectly well. In both of these cases, I believe the spasms were subdued by the sedative action of the electro-magnetic current upon the nervous system. Its anæsthetic action is also shown, as well as its effect as a cathartic.—*Dublin Medical Press, Nov.* 12, 1862, *p.* 474.

14.—ON DELIRIUM TREMENS AND ITS TREATMENT.

By Dr. William Perrie, Jun., Aberdeen.

[For the proper and rational direction of therapeutic measures, it is absolutely necessary to have a proper knowledge of the ordinary course and usual terminations of the diseases we undertake to treat. Now, in the vast majority of cases of delirium tremens, the paroxysm subsides spontaneously in from forty-eight to ninety-six hours, and profound sleep comes on, independently of art. This fact explains the apparent success following such various modes of treatment as have been at different times recommended. Bleeding is now entirely out of vogue. Graves and other subsequent writers have recommended the use of small doses of tartar emetic, to reduce the vascular excitement present in the disease. If we believe that there always occurs in delirium tremens, if not a real meningitis, at least a condition of the membranes closely bordering on it, this is a most rational plan of treatment. But this is not by any means certain, and similar appearances are sometimes observed in the brains of confirmed drunkards, who have never suffered from delirium tremens. The use of large doses of tincture of digitalis has been recently brought before our readers (vide *Retrospect*, Vol. xliv., *pp.* 29-35.) The free exhibition of some alcoholic liquor is perhaps the most generally adopted practice of all. By this plan Dr. Wood states that the proportion deaths in the Philadelphia Hospital was reduced from one in eight to one in about thirty-three. Dr. Perrie states, however, that after carefully noting the effects of this plan in cases under his own charge, he never yet saw an instance in which speedy improvement followed its adoption, or one in which any marked alteration for the better was distinctly referable to its employment. A continuance of this treatment is not warranted by the experience of those who have had unusually large spheres of observation.]

Although many and great authorities strongly advocate the employment of *opium* there is far from unanimity of opinion on the point, for many who are deserving of the highest regard and who have had large experience of the disease, strongly argue against its use. Dr. Morehead, in referring to the opium plan of treatment, tells us, that "a greater number thus treated terminate by convulsions and coma;" and Dr. Bennett, in speaking of the value of sleep, says, "opium has been largely given to obtain this result, but it is much to be doubted whether its supposed beneficial action is not dependent on coincidence with the muscular fatigue and exhaustion which, with the tendency to repose, accompanies the elimination of the alcoholic poison." "I have no hesitation," writes Dr. Peddie, in saying, "that in a large proportion of instances sleep would take place spontaneously at an earlier period, and the subsequent condition of the patient would be much more sound and safe, by doing nothing at all than by the use of opiates. It is evident, that if opium be used at all in delirium tremens, it must be given in a large dose (in from two to three or more grains, and repeated at intervals of a few hours) ; and it is thus generally given, the object being to overstep the stage of excitement, and force on the desired sleep. Now the acknowledged effect of a large opiate on the encephalon is to occasion engorgement of the vessels, more especially of the veins, and consequently, the larger the dose, the greater will be the amount of sanguineous compression of the brain. What then must be the probable result in a disease in which there is already, if not an approach to arachnitis, at least a very excited action of the meninges, and a preternatural loading of the vessels generally ? The practice is one of the utmost hazard." These remarks apply to opium whatever be the form in which it is administered ; whether in the form of enema as advocated by Dupuytren, when any contraindication existed to giving it by the mouth ; or of injection into the subcutaneous cellular tissue, as has been tried by Dr. W. Ogle, in this disease.

Drs. Macpherson, Laycock, Law, Cahill, and others, also testify to the use of opium being either unnecessary or injurious in the treatment of delirium tremens, so that it is clearly manifest that the high value attached to it by some in this complaint, is far from warranted by the experierce of many other high authorities.

Perhaps the favourite way of treating the disease in this country is by a combination of *opium and alcoholic liquors.* "If a man," says Mr. Phillips, "has been accustomed to drink largely of malt liquor, a drachm of laudanum will act much more beneficially if taken in a pint or pot of beer, than if taken alone. A similar remark may be applied to other spirituous liquors; and in other cases its effects would be most certainly enhanced if it were, as soon as practicable, associated with animal food." According to Dr. Billing, "the only mode of remedy is by narcotics and stimulants; by which, in addition to the counteraction of the sedative state, a greater tendency to sleep is produced." Dr. Cullen, in speaking of the value of opium in inducing sleep, says that it frequently fails to produce this effect in "habitual and enervated drunkards, and where the disease has followed the disuse of the usual quantity of stimulus, unless combined with some of the diffusive stimuli. In such cases, a moderate allowance of the accustomed stimulus in the form of wine, brandy, or malt liquor, as circumstances may point out, is sometimes attended with marked good effect in diminishing the restlessness, tremors, and other urgent symtoms." "Opium," says Dr. Armstrong, "is the main remedy, and good mutton broth, or beef-tea, is the best diet; with a tolerable quantity of good malt liquor as common drink;" and Dr. Watson advises us to "put their opiate dose into a glass of gin or a pint of porter." It has always appeared to me to be a most extraordinary proceeding to employ at one and the same time two substances, one of which is intended to soothe and calm the system, and the other is professedly given to stimulate and exalt it; but there is no use to enter into any argument against this mode of practice, because, if we have shown that alcoholic spirits and opium, when employed singly in the treatment of the malady, are each unwarranted both by theory and experience, it necessarily follows that their combination must be doubly injurious. Notwithstanding the general countenance given to the practice, there are not wanting many physicians of great experience and high standing, who have long felt thoroughly dissatisfied with it, and have recorded their opinions to that effect. Dr. Peddie tells us, "The frequent sudden fatalities which I witnessed from arachnitis, convulsions, and coma, when stimulants and opiates were freely administered, and the length of time ere recovery took place, even in the most favourable instances of the malady, when these agents were given more sparingly and cautiously, long since convinced me that their tendency is highly dangerous;" and Dr. Morehead says, "I can say nothing of the treatment of delirium tremens by free opiates and stimulants in temperate climates, but I feel myself satisfied in very positively asserting, that in the delirium tremens of Europeans in Bombay, it is a course of treatment attended with much hazard, and which, when systematically followed, is certain of leading to unfortunate results." Many other quotations expressing like views might be given, but these will suffice, as we must now hurry on to a cursory notice of the next two methods of treating the disease,—*the expectant and the eclectic treatment.* I prefer to speak of both these methods together, because both have been recommended and followed by the same physicians, their choice on each occasion being determined by the particular circumstances of each individual case. They have been followed out alike by those who consider an attack of delirium tremens to be the immediate result of refrainment, and by others who believe that the paroxysm is in no way owing to suspension of the habitual stimulus. Dr. Dunglison, of Philadelphia, believing that the nervous disturbance results from the withdrawal of an accustomed stimulus, but that the recuperative powers are generally sufficient to re-establish the healthy balance, was wont to treat his cases without either excitants proper or opiates. In some instances he adopted a purely eclectic, and in other instances an expectant plan of treatment. Sometimes, if the patient was suffering from the immediate effects of a debauch, and no counter-indication existed, an emetic was given; gentle laxatives were administered from time to time, nutritious and easily-digested food was enjoined, and too much light as well as noise were prohibited. At other times a strictly expectant practice was pursued. In the third edition of his Practice of Physic he informs us, that of thirty-two cases, eighteen of which were classed as intoxication, not one died; although the treatment was eclectic or expectant, and did not include a drop of spirits.

Dr. Laycock who holds that the outburst of the paroxysm is in no way owing to the withdrawal of the ordinary stimulus, advocates the expectant or the eclectic plan as the really rational method of treatment; and he gives as his experience, that of twenty-four cases admitted under his care in 1857, and of four private ones, one only was fatal which was treated with opium and alcoholic stimulants. Dr. Wood states that the treatment pursued by Dr. Kuhn of Philadelphia consisted "in confining the patient in a dark cell, and leaving the disease spontaneously to work itself off;" and Dr. Ware states that he is satisfied "that in cases of delirium tremens, the patient, so far as the paroxysm alone is concerned, should be left to the resources of his own system, particularly that no attempt should be made to force sleep by any of the remedies which are usually supposed to have that tendency." The same plan has also been followed by Calmiel and others ; but a sufficient number have been mentioned to show that this mode of practice, like every other that has been previously noticed, has many and great supporters.

From a review of all that has been said on the subject of delirium tremens and of its treatment, we gather the important facts that there is no unanimity of opinion as to the exact import of any one symptom of the disorder, that physicians are entirely divided on the subject of its ætiology, that the most conflicting accounts have been given of its pathology, that the most contrary modes of treatment have been advocated and adopted, and that any one of these various plans could be favourably illustrated by the experience of some, though not of other practitioners. The force of this last remark will at once be seen by a glance at the following brief statistical table :—

By purely stimulant plan in the Philadelphia Hospital, . .	Proportion of Deaths reduced from 1 in 8 to 1 in 33.		
Treatment by opium and alcholic drinks in Edinburgh Royal Infirmary during three years and a quarter, . . .	Cases admitted. 403.	Deaths. 101.	Proportion. 25 per cent.
Tartar emetic in sedative doses, by Dr. Peddie, ...	Cases treated. Upwards of 80.	Results. Uniform success.	
Large doses of digitalis, by Mr. Jones, ...	Cases in which it alone was used. 67.	Results. 1 died.	
Eclectic or expectant plan, Dr. Laycock, ...	Cases. 28.	Results. 1 died from using opium and stimulants.	

It is quite apparent that the profession in general are quite dissatisfied with the limited influence they can yet boast of over the disease ; and the continual trial of new plans, and the eager hunt after new remedies, clearly indicate the belief that more might be effected than can yet be said to be the case. Considering the utterly conflicting nature of the opinions entertained on every point connected with the disease, and considering the many plans of treatment proposed, and the discrepant results attributed to each by different practitioners, it must be desirable that all who have had any opportunities of familiarizing themselves with the disorder should state their views as to its nature and proper management, so that from the experiences and opinions of many, some rules of conduct might be divulged which would be generally applicable to the treatment of the disease.

I cannot myself discover in the complaint any indications of vascular excitement approaching to inflammation, neither can I look upon it as simple nervous exhaustion, nor as a chain of phenomena symptomatic of simple gastric derangement. I am, therefore, entirely opposed to venesection and to regular antiphlogistic treatment, and have not been able to see, on the grounds either of theory or of experience, the particular call for sedative medicines. I cannot but confess that to me the use of stimulants and opiates appears unsupported by experience, and not warranted by theory; and from the views I myself entertain, I think that a step further might be taken in the way of treatment than is expressed in the writings of those who have made favourable mention of the expectant and eclectic plans. The expectant plan of course implies that little or nothing is done which could in any way influence the complaint; but the term eclectic implies the possibility of all kinds of treatment being tried at some time or another, though not in the routine way in which each of them is carried out by its warmest supporters.

I believe that this disease is a specific form of nervine poisoning, of which alcohol is at once the predisposing and the exciting cause; that the blood is poisoned by alcohol either unchanged or combined with some material generated in the system; that the blood is rendered perfectly unfitted for the proper nutrition of the brain by long want of proper food, and by the retention of numerous matters which, in a state of health, are at once excreted either unchanged or after undergoing some metamorphosis; and that, therefore, the proper indications for treatment are, not to trust entirely to nature, or to treat some accompanying disorder, but at once to cut off any further supply of the poisonous substance by which the blood is infected, to neutralize (if it be possible), or to counteract the influence of that which has already gained admission into the system; to strive by all means to get nourishing food administered; to take proper measures for the thorough depuration of the blood, and the elimination from the system of the many effete matters with which we have seen that it is crowded, by means not depressing to the vital powers, or calculated to favour cerebral congestion; and to obviate the tendency observed to death.

I have already said so much with regard to the sudden withdrawal of the favourite stimulus, that it is altogether unnecessary again to revert to the subject. But the mere prevention of any further admission of the toxic agent into the system is not the great end of treatment in other forms of poisoning, and I cannot see why it should be so in the case in question. It is no argument against our proceeding a step further, to say that the disease so often recovers of itself. I readily grant this, but I may at the same time state that measles and scarlatina very frequently recover of themselves, and yet there are few who do not take some steps to eliminate the poisons of these diseases from the system. The next question, therefore, comes to be, do we possess any immediate antidote to the poisonous effects of alcohol? Opium and belladonna are said to have mutually antidotal properties; and some have claimed for digitalis the virtues of an antidote to the poisonous influence of alcohol. This, however, is a hasty and unsound conclusion, unwarranted by the most favourable of the cases recorded in illustration of of the good services of digitalis, and altogether negatived by the experience of others who have given it a trial. We as yet know of no substance which can at once restore the sufferer from delirium tremens to soundness of mind or body, and it does not appear very rational to expect such a sudden change in this disorder when we reflect that, whether alcohol itself exists in the blood free or combined, yet that fluid has been rendered highly poisonous and quite unfitted for healthy nutrition by the accumulation in it of numerous effete matters, owing to the prevention, by the alcohol, of those changes necessary for their removal from the system. It naturally follows from what has been said, that we cannot look for much improvement in the condition of the sufferer, until his blood has been freed to a certain extent of these noxious ingredients, and made richer in quality by the persevering exhibition of proper food, and so rendered more suited for the purposes of nutrition. I therefore think that an important and easily fulfilled indication in every case, is to help the depuration of the blood and the support of the system by the natural stimulus of nourishing food. In every blood disorder we can see more or less decided marks of natural eliminative efforts, or attempts to cast out of the system that by which its healthy nutrition has been prevented. Thus, in scarlatina and in rheumatic fever we see in the state of the skin a natural effort to get rid of the poison; and I look upon the profuse perspiration in delirium tremens, not as the mere result of excessive muscular action, but as one of the natural means by which the depuration of the blood is effected. The body during the whole paroxysm is more or less covered with sweat; but we may often observe in cases with a favourable termination that, after sound sleep has been established for some time, a remarkably profuse sweat breaks out, and the patient lies for a considerable time literally bathed in it. In all cases, therefore, I deem it of importance to maintain a proper temperature, and to encourage free action of the skin. When the cutaneous surface is warm and moist, we need not interfere; but if the temperature becomes decidedly low, and more especially if, along with this, there be a decided diminution or arrest of the cutaneous exhalation, the use of stimulating diaphoretics is clearly indicated. Of all this class of medicines, I think a combination of camphor and ammonia is most suitable, both on account of its decided influence over the cutaneous secretion, and also from its diminishing the excessive acidity I have so often observed in the complaint; from its power of exciting and strengthening the enfeebled circulation; from its affording a distinct fillip to the nervous system without inducing subsequent depression; and from it exerting no influence over the intellectual faculties.

The depuration of the blood may be further promoted by arousing with suitable medicines the hepatic and other secretions, which are more or less retained both prior to, and during the paroxysm. The kidneys afford one of the best channels for the elimination of the effete materials accumulated in the blood; and these organs are easily incited to increased energy by any of our common unirritating saline diuretics, with or without ammonia, according to the degree of asthenia manifested in each particular case. Throughout the whole period of treatment strong broths or beef-tea should be regularly administered; and exhaustion counteracted by this, the best, because the natural form of support.

The *eliminative and supporting plan* then is the one which a careful study of the symptoms, pathology, and course of the disease, have suggested to me as the most likely to further the natural method of cure, and the one which, by its depurative and derivative, but not debilitating character, is most calculated to avert convulsions, coma, and the other not unusual accidents of the disease. There are just two other points to which I would refer before drawing my remarks on the treatment of uncomplicated delirium tremens to a close. In all cases, I think it a useful measure to maintain the application of cold to the head as steadily as circumstances will admit, and to endeavour, without resorting to violence, to restrain the movements of the patient as much as possible. Here again a directly opposite plan has been recommended and favourably illustrated. Dr. Blake, while in the West Indies, found that attacks of delirium tremens were effectually averted by giving proper nourishment, and by enforcing "walking drill," which was supposed to do good not as a change of stimulus, but by exerting an exhausting and eliminating effect; and Dr. Cahill mentions cases in which sleep ensued after fatiguing drives enjoined as a part of treatment. Some have advocated freedom of bodily movement, on the plea that the patient expends much more strength in trying to free himself from his attendants, than when allowed to walk about at pleasure in a properly secured apartment, or under the care of a constant attendant. I have always made it a point to have the sufferer strictly confined to bed, and have never had any difficulty in carrying it out without resorting to the slightest violence. All that is required is to get a sufficient number of the patient's relatives or most intimate acquaintances to undertake the task for allotted periods; and I have always found that the patient comes to submit, without struggling, to their gentle restraint, although such would not have been the case with perfect strangers. If it be possible to restrain the patient without displeasing him, it follows that much exhaustion may be avoided, and a better chance given to other medicines to act satisfactorily. It has always appeared to me a most strange thing, that they who employ opium and alcoholic stimulants should also permit a patient to exhaust himself by uninterrupted muscular effort. It is consistent in those whose aim it is to induce exhaustion and lower the systemic vigour; but that the giving of opium to soothe and put to sleep a patient, and at the same time plying him with spirits to support and stimulate him, and all the while allowing him to rove about and exhaust himself, are equally consistent measures, is perhaps not so easily seen.

I have witnessed a great many examples of the disease both here and elsewhere, and have seen enough to give me a very unfavourable impression of the opium and alcoholic plans of treatment. But I would not advert to cases of which I do not possess notes, and of whose history I am not fully aware. Of eleven cases treated in private, four were put upon opium and alcoholic stimulants, and the remaining seven treated by the eliminative and supporting plan which I have just recommended. Of the four cases treated by opium and alcoholic stimulants, two died; and in the other two, the paroxysms were protracted to the fifth day, and recovery was very tedious. The other seven cases, severe ones, treated by the eliminative and supporting plan, all recovered within the fourth day; the patients being thoroughly cured within from fifty-four to ninety-four hours.

I would not advance this as any proof of the great superiority of the latter plan of treatment, as delirium tremens is one of those diseases in which there is such a strong natural tendency towards recovery, that any particular plan of treatment would require to be tried in a larger number of cases than occur in any single person's experience, before a trustworthy conclusion can be arrived at as to whether it really shortens the paroxysm, or really diminishes the mortality from the disease.

My own views of the complaint, and my past experience, satisfy me that delirium tremens should be treated on the same principles as other blood disorders, and that the eliminative and supporting plan is the really rational one, and the most calculated to shorten the paroxysm, to mitigate the symptoms, and to prevent those casualties which are fraught with greater danger to the sufferer than is the original disorder itself.

In conclusion, I would remark that though I have spoken thus strongly in favour of the eliminative and supporting plan of treatment, and against the practice by opium and alcoholic stimulants, I do not mean that I consider delirium tremens to be a disorder in which it can never by any possibility be proper to give a moderate allowance of the previous stimulant. In delirium tremens, just as in any other disease, typhoid symptoms may take the place of all others, and become so pronounced, and continue so unabated after the use of ammonia or any other simple stimulant, as imperatively to demand our resorting to the use of spirits. But then this is but a usage of alcoholic stimulants which obtains in the treatment of all diseases, and does not constitute a distinct plan by itself. Nevertheless, I cannot see how they can ever be of the least service until the third or fourth day of the disease, until the system has had time in some measure to recover from a state of alcoholism. Should typhoid symptoms occur early in the disorder, I fail to see how they can be relieved by the administration of alcoholic stimulants, seeing that the system is already so fully under their influence as to have become poisoned by them and altogether unfitted for the proper discharge of its functions.—*Edinburgh Medical Journal, Dec.* 1862, *p.* 509.

15.—DIGITALIS IN DELIRIUM TREMENS.

By W. B. Kesteven, Esq., Upper Holloway.

In a case of delirium tremens, which occurred to me a short time since, I administered three separate doses, one drachm each, of tincture of opium, without the least benefit, stimulants being freely given at the same time. Finding that no good result followed, I gave four doses, of half-a-drachm each, of tincture of digitalis. The patient continued still excited, not having closed his eyes for ninety hours. The relatives then requested a consultation.

Two hours after the last dose of tincture of digitalis had been given, Dr. Ballard met me at the patient's bedside, when we found that he had just fallen into a quiet sleep. This lasted more or less completely for thirty-six hours, and the patient was apparently safe. He was a man of middle age and an habitual toper. It was his first attack. He had, however, a relapse a few days afterwards. I at once gave him two drachms of tincture of digitalis, to be followed every two hours by half-drachm doses. In less than three hours he was soundly asleep. Several weeks have since passed, and there has been no return of the malady. I would add, that in the next case of delirium tremens that falls to my lot, I shall be disposed to begin with a larger dose than I gave in this case.—*Med. Times and Gazette, Feb.* 28, 1863, *p.* 210.

16.—DIGITALIS IN DELIRIUM TREMENS.

By Dr. Robert Duchesne, Woodford.

The effect of large doses of digitalis in a case of "delirium tremens," which occurred in my practice a short time since, tends powerfully to corroborate the theory of its action, as held by Dr. Handfield Jones in his communication in your last Number. The case was briefly this:—

E. F., twenty-eight, a prostitute and confirmed drunkard, first seen in her own house at 7 p.m. She had taken no food, except a little milk, for three days; has been drinking beer and spirits; fancies herself dead; surface of body is cold; pulse 90, soft and small; secretions, as well as can be ascertained, normal. To take ℥ss. tinct. digitalis immediately; the same to be repeated in four hours; and subsequently ʒij. until the desired effect be induced.

9 a.m. Has slept six hours and taken two quarts of milk; pulse 84, full and natural; surface warm; is thirsty; quite sensible, and willing to take beef-tea and continue the milk; has had three doses of the digitalis; excretions natural.

On the following day, there was some little nausea and slight delirium, but these yielded readily to ordinary remedies and slight stimulation. There was no relapse, and the case ended most favourably.

There can, I think, be no doubt that digitalis acted here the part of a "cardiac tonic" in a case of decidedly "asthenic," delirium, &c.—*Med. Times and Gazette, Jan.* 3, 1863, *p.* 24.

17.—ON HYDROCYANIC ACID IN THE TREATMENT OF INSANITY.

By Dr. Kenneth Mc'Leod.

[A patient was admitted into the asylum, the most prominent symptoms being "violent shouting, singing, spitting, and biting." She was aged 53, and had led a most dissipated life. She was in an extremely feeble condition; wasted, weak, and anæmic. There is incessant muscular motion of every possible kind, of tongue, feature, and limb. The further report of the case is as follows.]

July 9th. No improvement whatever; is rather worse; more noisy, and restless; is incessantly in motion; constantly shouting, screeching, &c.; tosses off the clothes; gets out of bed whenever she is left alone; no sleep at all last night. To have—℞. Acid. hydrocyani dil. ♏iv., every hour in ℥ss. aq. camph.

10th. Almost immediately after the first dose became quiet and still; effect sustained during the whole of the afternoon; slept well last night, a very marked contrast to several nights preceding; is much more settled this morning; does not shout or scream, although conversation is unconnected and rhapsodical; or takes her food better.

12th. Improvement still sustained; is much more manageable; eats her food better; is cleaner in habits; keeps her bed; ward generally quiet.

14th. Is not so well; rather more restless and noisy; is still affected for the better by the hydrocyanic acid, but not so much as formerly.

The acid was about this time discontinued from failure of the supply of it, and she relapsed to nearly her former condition, being unaffected by large doses of hyoscyamus and morphia. It was resumed again towards the end of June, and again she improved amazingly; was able to get up every day; was generally quiet, but occasionally became excited; her physical condition, notwithstanding the administration of wine, porter, and a generous diet, gradually declined; she emaciated more and more; feet became œdematous, and bones every day got more prominent. She again became disorderly and acutely maniacal on remission of the drug, and was a third time habitually quelled by it.

She lingered on till August 16th, an attack of pneumonia accelerating her death, and died almost a perfect skeleton.

On post-mortem examination, the optic nerves were found completely atrophied, the optic tracks partially so, and only the posterior transverse bands of the commissure remaining. Brain showed signs of meningeal irritation, and was the subject of chronic congestion throughout. There was no softening or any other structural change. The liver and kidneys were very gravely degenerated; capsules and spleen softened, and heart dilated; right lung solidified.

Maniacal Melancholia Coincident with Anæmia and Cachexia—an Acute Paroxysm Treated Successfully by Hydrocyanic Acid—Gradual Improvement under Tonics and Dietetics—Recovery.—H. S., aged twenty-nine, married, admitted December 12th, 1861. No history, except the facts contained in the reception order. They are—Labours under a second attack of four months' duration. The first occurred at the twenty-eighth year of her age. Cause not known. Is suicidal and dangerous to others. In the latter part of October attempted suicide by cutting her face and neck with a razor. Imagines her domestic affairs are disordered. Has not slept for more than an hour at a time during the last six months. Is restless, incoherent, and occasionally very desponding.

Symptoms on admission.—Patient inclines in diathesis to a strumous type. Is in very delicate general health, anæmic, and melasmic. Has not menstruated for months. Labours under maniacal melancholia, deploring with tears and lamentations the loss of everything; contrasting former comfort and happiness with the blighting ruin which involves not only her own affairs but every object in the world. Trees are withering, houses falling, property destroyed, cattle dying, &c., she the cause of all.

April 5th, 1852. Treatment since admission has been tonic, sedative, and dietetic. Has not improved much. Is very much pre-occupied with delusion; will not work at all; repeats the same expressions, conduct, and acts every day, without the slightest variation. 2 p.m. Is in a state of frantic and unrestrainable excitement—shouting, struggling, and throwing herself about with great violence; crying aloud in her usual strain, but with greater intensity. Is exceedingly emotional. Pulse 144. ♏ij. dilute hydrocyanic acid was administered instantly, but no effect appearing, the dose was repeated in five minutes. This was followed by a well-marked gradual amendment. Screaming, struggling, emotion, etc., disappeared by degrees. In ten minutes she was left alone, and in fifteen minutes became perfectly quiet. Pulse 120. In thirty minutes she continued silent and still, looking quietly out of the window. Medicine was continued every hour. She remained perfectly quiet during the afternoon; pulse falling to 96.

October. No such paroxysm as that related above has occurred again. Patient is now very much better; cheerful, industrious, stout, and ruddy. Change for the better induced principally by means of tonics, iron and quassia, and generous diet.

General Paresis—Extreme Excitement Rapidly Quelled by the Use of the Hydrocyanic Acid—Habitual Quietness secured by Prolonged Treatment.—W. D., a stalwart man, of arthritic diathesis, middle-aged, by trade a gas-fitter, was admitted June 3rd, 1861, labouring under the general paresis of the insane. On admission the physical signs of paresis were slight and mental, consisted in an incapacity of work, a bouncing manner, self-complacency, and self-confident boasting familiarity, with occasional emotional paroxysms. The disease became soon more intense; vocalisation became more impaired; appetite voracious. He became liable to excitement, stole, accumulated, repeated, and began to entertain extravagant optimistic delusions.

June 9th, 1862. Disease has become very much more acute. He is liable to the most fearful paroxysms of excitement, when, with raised look and frantic gesture, he rushes about screaming, and shouting incoherently of his wealth, power, position, importance, means, etc. Property he has without limit—40,000 guineas, ship-loads of parrots, an inconceivable quantity of anything that can possibly be imagined. Deeds he performs of the most miraculous description,—"goes to Italy and knocks down the rocky mountains;" does wonders "at the Crimean war;" talks of marrying Mary Magdalene, the mother of God, etc. His nights are most boisterous and disturbed. He gets out of bed, tosses his bed and bedding about, knocks at the door, and shouts incessantly.

10th. Had a most dreadful night; shouted and screamed during the whole of it; got out of bed and battered at the door with might and main; besmeared himself with fæces; could not be pacified by the night attendant. ℞. Acid. hydrocyanic dil. ♏iv. every hour.

11th. Became perfectly quiet towards the afternoon; was obviously affected by the first dose of his medicine, and decidedly quelled by the second. Slept soundly all night. Is quite another man this morning; sits quietly on his seat, and does not talk except when addressed.

13th. Continues to improve. The change of conduct during the night is most wonderful. He lies in bed and sleeps. No less remarkable is his altered demeanour during the day.

September 24th. The acid was on one occasion discontinued, when the patient reverted to his former violence of manifestation. Under prolonged treatment by it, he has become habitually quiet and tractable. Nothing similar to the former excitement has at all appeared.

October 22nd. Continues much the same, disease making slow progress. The excitement appeared again, but was promptly dismissed by another administration of prussic acid.

November 1st. No recurrence of excitement. Is somewhat stouter; looks better. Occasions no disturbance, and gives very little trouble.—*Med. Times and Gazette, March* 21, 1863, *p.* **290**.

DISEASES OF THE ORGANS OF CIRCULATION.

18.—A GENERAL ACCOUNT OF VALVULAR LESIONS OF THE HEART.

By Dr. Austin Flint, Professor of the Principles and Practice
of Medicine at Bellevue Hospital Medical College,
New York.

Valvular lesions are varied in their character. I cannot consider them fully in this connexion. They are due to atrophy of the valves, rupture, atheromatous and calcareous degeneration, warty growths, etc. On the table before me are several specimens. Suffice it to say, that the varied lesions may produce, *First*, contraction of the mitral or aortic orifice. Lesions producing this effect are called obstructive lesions; they obstruct the direct blood-currents. *Second*, they may impair the aortic valvular segments or the mitral valvular curtains, and render them to a greater or less extent inadequate to protect the orifices. The valves are then said to be insufficient or incompetent, and the orifices are said to be patescent. Consequently, regurgitation ensues, and then we have regurgitant blood-currents. If the mitral valvular curtains are damaged, we have the *mitral regurgitant current.* This abnormal current is from the ventricle to the auricle. When does it take place as regards the heart-sounds? The current is produced by the contraction of the ventricle; and since the contraction of the ventricle causes the first sound of the heart, this current, of course, is with that sound.

If, on the other hand, the aortic valvular segments are rendered incompetent, we have the *aortic regurgitant current.* This current is from the aorta into the left ventricle. When does it take place as regards the heart-sounds? The current is due to the recoil of the arterial coats; and as we have seen that it is this recoil which causes the second sound of the heart, this current is with that sound.

Third, Valvular lesions may exist without involving either obstruction or regurgitation; they may simply present a roughened surface to the blood-currents in their direct course. Under these circumstances they give rise to physical signs. It may seem that I am needlessly minute and tedious in these preliminaries to the consideration of the diagnosis of valvular lesions. But I am not so. Without a clear apprehension of, and familiarity with the blood-currents just named, and their relations to the heart-sounds, the signs on which the diagnosis of valvular lesions is based, cannot be made intelligible; and with this preparatory acquaintance with the blood-currents, the diagnostic signs of valvular lesions are at once easily understood.

Let me, therefore, recapitulate the four currents; and I would urge you to repeat them sufficiently to render them perfectly familiar. Arranging them now into two classes, viz., the mitral and aortic, we have *First*, the mitral direct and the mitral regurgitant current; *Second*, the aortic direct and the aortic regurgitant current. The mitral direct current, produced by the contraction of the auricle, is pre-systolic, that is, it occurs just before the first sound of the heart. The mitral regurgitant current, produced by the contraction of the ventricle, occurs with the first sound of the heart. The aortic direct current, produced by the contraction of the ventricle, occurs with the first sound of the heart. The aortic regurgitant current, produced by the recoil of the arterial coats, occurs with the second sound of the heart. Adopting another classification, we may arrange the different currents into, *first*, those occurring with the first sound of the heart, and *second*, those occurring with the second or before the first sound; the former we may call the *systolic*, and the latter the *diastolic* currents. The systolic are the mitral regurgitant and the aortic direct current. The diastolic are the mitral direct and aortic regurgitant current.

I come now to consider signs of disease incidendal to the blood-currents just considered. In health, as we have seen, we have only the direct currents, and these, owing to the smooth and polished membrane which lines the valves and orifices of the heart, occasion no sound. We have only, in health, the two heart-sounds which have been considered. If, however, the orifices through which the currents flow be contracted by obstructive lesions; if the valves are rendered incompetent by lesions so that regurgitant currents take place, or even if the endocardial membrane be roughened by calcareous deposits, vegetations, etc., adventitious or abnormal sounds are produced. These abnormal or adventitious sounds are called *murmurs*. Here is a conventional use of terms which is to be borne in mind. The two sounds of the heart, either preserving their normal characters or modified by disease, are known as the heart-sounds; but all sounds which are originated by disease—adventitious—are distinguished as cardiac murmurs. The *murmurs* produced by the blood-currents within the heart are further distinguished as *endocardial murmurs*, and murmurs without the heart, that is, on the heart's surface, are distinguished as *exocardial*, or, sometimes, *pericardial* murmurs.

The murmurs produced by the blood-currents, or the endocardial murmurs, differ as regards intensity, pitch, and quality. They may be more or less intense, varying from such a degree of fulness that they are with difficulty appreciated, to such a degree of loudness that they may be heard at a distance from the chest. They may be high or low in pitch. In quality they are said to be soft, rough, and musical. Generally they are soft, and resemble the sound produced by the air coming from a pair of bellows; and then, they are said to be bellows or blowing murmurs. Sometimes the quality of sound is expressed by such terms as rasping, filing, grating, croaking, and then the murmurs are said to be rough. In some rare instances (I happen to have an example now in my hospital wards) they have a musical intonation. I may remark here that the intensity, pitch, and quality of the murmurs have not, in general, much pathological or diagnostic significance. Their value in the discrimination of valvular lesions depends chiefly on other points, and the first point is always to determine by which of the four blood-currents is a particular murmur produced.

Continuing to limit our attention to the left side of the heart, each of the four blood-currents which we have considered, may give rise to a murmur, either soft, or rough, or musical. There are, therefore, four cardiac murmurs: two produced at the mitral and two at the aortic orifice. These murmurs are distinguished by the same names as the blood-currents. We have a mitral direct and a mitral regurgitant, an aortic direct and an aortic regurgitant murmur. These murmurs denote the existence and the situation of valvular lesions. Whenever we find an endocardial murmur, we desire, in the first place, to determine which one of these four murmurs it is. How do we determine this? By ascertaining in the first place, its relation to the heart-sounds. The relation of the several murmurs to the two sounds of the heart are, of course, the same as the relations of the blood-currents. Consequently, a mitral direct murmur will immediately precede the first sound, and the mitral regurgitant murmur will accompany and follow that sound; the aortic direct murmur will also accompany and follow the first sound, and the aortic regurgitant murmur will accompany and follow the second sound of the heart.

In all cases, after having ascertained the existence of a murmur, or murmurs, the first thing to be done is to determine when the murmur, or murmurs, occur as regards the heart sounds. In order to do this, of course it is requisite to discriminate the first and second sound. Usually the rhythm of the sounds suffices for this discrimination: the two sounds succeed each other with a scarcely appreciable interval, and then follows a distinct interval. The rhythm, I repeat, usually suffices, and, in addition, we have, frequently the distinctive characters of each sound to aid in the discrimination. But in some cases it is not easy to decide which is the first and which the second sound. This occurs when the heart acts with great rapidity, and the characters which distinguish the sounds from each other are, in a great measure, lost. I have a case of this kind now in one of my wards. In that case, as you will see when I call your attention to it at the bedside, the sounds recur so quickly that the normal rhythm is not readily appreciable, and the first sound differs but little from the second. Now, in such cases, there are two modes which we may employ in determining which is the first and which the second sound. One mode is to place the finger over the apex, and feel the apex-beat while we listen to the sounds. The sound which we find to be synchronous with the apex-beat is, of course, the first sound. This mode is not always available, for the apex-beat is not appreciable in all cases. The other mode is more uniformly available. It consists in placing the finger over the carotid artery, and observing which sound occurs synchronously with the carotid pulse. The carotid pulse and the first sound are synchronous. This is not true of the radial pulse; an appreciable interval elapses between the first sound and the pulse at the wrist, and hence the latter is not a guide.

The mitral direct murmur takes place just before the first sound of the heart, the apex-beat and the carotid pulse; the mitral regugitant murmur and the aortic direct murmur accompany and follow the first sound, the apex-beat and the carotid pulse; the aortic regurgitant murmur accompanies and follows the second sound of the heart. Of these murmurs, two denote, from their relations to the heart-sounds alone, the situation of valvular lesions, viz. the mitral direct and the aortic regurgitant. The remaining two murmurs occurring in the same connexion with the heart-sounds, other points are necessary for their discrimination; and, with regard to all the murmurs, other points than the time of their occurrence are brought to bear upon their discrimination. Before proceeding to speak of these, let us consider how far the murmurs are capable of leading us in the diagnosis of valvular lesions.

Valvular lesions may involve, first, obstruction of the mitral and aortic orifices; second, insufficiency of the valves protecting these orifices, and consequent regurgitation; and, third, mere roughening of the surfaces without either obstruction or regurgitation. In the latter case the lesions are comparatively innocuous; they are serious, other things being equal, in proportion to the amount of either obstruction or regurgitation, or both combined. Now, the murmurs generally enable us to determine, in the first place, their situation, whether in the aortic or mitral orifice, or in both these situations; in the third place, the existence of valvular insufficiency or of regurgitation; and, in the fourth place, in some cases, the existence of obstruction. These four points of information are not only of interest, but of great practical importance. The murmurs thus lead us a good way into diagnosis, but still not so far as we could desire, and as we are able to go by combining with the knowledge afforded by the murmurs, facts pertaining to other signs and symptoms. The murmurs alone do not afford us much information respecting the amount of obstruction or regurgitation, and consequently of the gravity of the valvular lesions.

Proceeding to consider more fully the diagnosis of valvular lesions, we will take up first those seated at the mitral orifice. Here is a specimen illustrating both contraction and patescency of this orifice. It was taken from the body of a patient who died at Blackwell's Island Hospital. The patient was examined by me, and the diagnosis made some time before his death. There are two murmurs emanating from this orifice, viz. the mitral direct and the mitral regurgitant. The mitral direct murmur generally denotes mitral obstruction. With some exceptions, to which I shall hereafter refer, we base the diagnosis of obstructive lesions situated at the mitral orifice, on the presence of this murmur. I wish you to take notice of this murmur especially, for the most distinguished auscultators deny its existence. When the points involved in its recognition are clearly apprehended, it is as easily recognised as either of the four murmurs. No other murmur occurs in the same relation to the heart-sounds ; it is, as we have seen, pre-systolic in the time of its occurrence. I have three cases under my observation in Bellevue Hospital presenting this murmur. Aside from its being pre-systolic, there is generally a distinctive character in the sound itself. It is usually somewhat rough, resembling the sound produced by throwing the lips into vibration with the expired breath. When this character of sound is strongly marked, I have called it a blubbering murmur. This character of sound I suppose to be due to the vibration of the curtains which compose the mitral valve. It is heard with its maximum of intensity around the apex of the heart. Often it is limited to a small space. It is propagated best in a direction from the apex anteriorly towards the median line, not so well in an opposite direction. Sometimes it is so loud as to be heard over a considerable area. These are the points involved in the discrimination of this murmur. Its presence, as I have said, denotes, with some exceptions, the existence of mitral obstructive lesions ; but its absence is not proof that these lesions do not exist. It is present in only a certain proportion of the cases in which obstructive lesions exist ; and it is especially apt to be present in one variety of these lesions, viz. when the mitral curtains become adherent at their sides, and form what is known as the button-hole contraction.

A regurgitant stream of blood through the mitral orifice always gives rise to a murmur unless the action of the heart be exceedingly feeble. The mitral regurgitant murmur, as we have seen, accompanies the first sound; it is one of the two systolic murmurs, the aortic direct murmur being the other. How is the mitral regurgitant to be discriminated? It has its maximum of intensity at or near the apex of the heart. It may be limited to a circumscribed space in this situation; but if not thus limited, it is propagated best and furthest in a lateral direction around the chest, towards the spine. It is often heard on the back, to the left, and not unfrequently to the right of the spinal column. It is propagated better in this direction than upwards, towards, and above the base of the heart. It is not heard over the carotids. The murmur may be more or less intense, soft, rough, or musical. We shall see presently that the points just stated with respect to the maximum of intensity, the direction of propagation, and absence from the carotids, suffice for the discrimination of this murmur. It is so constantly present when mitral regurgitation exists, that we may, in general, infer from its absence that regurgitation does not occur.

Does the mitral regurgitant murmur always denote actual regurgitation? You will say, "of course it should, from its name, have this significance." Yet it does not. We may have a mitral regurgitant murmur without regurgitation; that is, a murmur with the first sound referable to the mitral orifice may be produced when there is no insufficiency of the mitral valve. Roughening of the mitral curtains will produce it. It is therefore more accurate to call this murmur a mitral systolic murmur, with or without regurgitation. Can we determine, during life, whether the murmur be a regurgitant or non-regurgitant murmur? I think we can do this in many, if not most instances. If the murmur be limited to a circumscribed space, at or within the apex of the heart, that is, not propagated much without the heart towards the left lateral surface of the chest, and not heard on the back, it does not denote regurgitation. On the other hand, it does denote regurgitation, as a rule, if it be propagated much without the heart, and if it be heard on the back. To this rule, however, I believe there are some exceptions.

The two mitral murmurs occur separately or in combination. The mitral systolic occurs much oftener without than with the mitral direct murmur. Of the four murmurs this is the one most frequently met with in practice. The mitral direct, also, occurs alone. This is true of two of the three cases now under our observation. It then denotes obstructive lesions without regurgitation. In the remaining case both murmurs are present, denoting both obstruction and regurgitation. In this case we have a rough pre-systolic and soft systolic murmur at the apex, the first sound dividing the two murmurs.

To understand the diagnostic symptoms of mitral obstructive and regurgitant lesions, you must consider the effects of these lesions on the heart. Marked symptoms are not produced until the lesions have led to enlargement of the heart, and, in giving rise to enlargement, the first effect is on the left auricle. The left auricle becomes over distended and, at length, dilated from the accumulation of blood. Then ensues pulmonary congestion, next follows hypertrophy, and finally dilatation of the right ventricle, and last, the right auricle becomes dilated. The left ventricle may become but little or not at all enlarged. You see these effects on the heart finely exemplified in the specimen which I have already exhibited in this lecture, showing mitral contraction and insufficiency. The left ventricle is scarcely increased in size; the left auricle is much dilated; the right ventricle is hypertrophied and dilated so as to form a much larger portion of the organ than the left ventricle; and the right auricle is dilated. Now, as one of the symptoms of mitral lesions, involving considerable contraction or patescency, or both, we have weakness of the pulse. The pulse is weak because, in proportion to the amount of obstruction, the left ventricle is im fectly supplied with blood, and, in proportion to the amount of regurgitation, blood is impelled backwards into the auricle which would otherwise pass into the aorta with each ventricular systole. Weakness of the pulse is thus, to some extent, a criterion of the amount of obstruction or regurgitation at the mitral orifice, especially if the action of the heart be not notably weakened, and the contrast, in some cases, between the pulse and the heart's impulse is very striking. This contrast is strongly indicative of mitral lesions.

Obstruction to the passage of blood from the lungs to the heart, arising from the accumulation of blood in the left auricle, involves pulmonary congestion. Hence, cough and expectoration are more or less prominent symptoms. Hæmoptysis, cepecially if there be much mitral contraction, frequently occurs from time to time, the amount of hemorrhage being usually small. Pulmonary apoplexy is liable to occur under these circumstances. Dyspnœa is a distressing symptom in proportion to the degree of pulmonary congestion, and when the right ventricle becomes weakened by dilatation, this becomes the source of great distress. At first the patient only experiences want of breath on exercise, but in an advanced period of the disease the suffering may become constant and extreme.

It is rare that patients suffer much from pain in the præcordia; they complain of a sense of oppression and indefinite distress, but not of actual pain. Palpitation is not a source of much annoyance. The appetite and digestion may not be notably affected. Nutrition may be but little impaired; patients do not emaciate. The urine may contain a small quantity of albumen, indicating congestion of the kidneys, and not necessarily degenerative disease of these organs.

When dilatation of the right cavities of the heart has taken place, the veins of the neck are frequently turgid, and the surface of the body presents a cyanosed hue more or less marked. With or or without turgescence of the veins of the neck, in some cases undulatory movements of these veins are observed, sometimes striking and regular, constituting a venous pulse. In some rare instances the pulsation is apparent to the touch as well as to the eye. These undulations are due to the contractions either of the right auricle or the right ventricle, or to both. It is generally easy to determine whether the jugular pulse be auricular or ventricular, or both combined. First, you satisfy yourselves that the pulsations are venous by pressing gently on the veins of the lower part of the neck, so as to interrupt the flow of blood to the heart. If the pulsations are venous they will be arrested, and return immediately the pressure is removed, the pressure being quite insufficient to arrest the flow of blood in the arteries of the neck. Having determined that the pulsations are venous, you place a finger over the carotid artery on the opposite side, and compare the visible pulse in the vein with the arterial pulse which you feel. Sometimes the pulsations in both the veins and arteries are visible. If the venous and arterial pulses are synchronous, the venous pulse is due to contraction of the right ventricle; it is a ventricular venous pulse. But if the venous pulse just precedes the arterial, as the contractions of the auricle precede those of the ventricle, it is auricular. And if for each arterial pulse there are two pulsations of the vein, one synchronous with the arterial pulse and the other preceding it, the auricular and ventricular venous pulse are both present.

Dropsy occurs when the right cavities have become dilated. The dropsy depends on congestion of the systemic veins, arising from the obstacle to the return of blood to the heart. It commences usually in the lower extremities, and the œdema here may become very great if the patient be unable to lie down in consequence of orthopnœa. At length it extends over the body, and effusion takes place into the serous cavities.

Death takes place gradually in the great majority of cases. Instances of sudden death are not very frequent in cases of mitral valvular lesions. The usual mode of dying is by slow apnœa. Patients are liable to be destroyed by œdema of the lungs, pulmonary apoplexy, or some intercurrent affection.

These lesions, like the mitral, may be obstructive or regurgitant, or both obstructive and regurgitant; or, lastly, neither obstructive nor regurgitant, in other words, innocuous. Here is a recent morbid specimen illustrating lesions affecting the aortic valves, so as to involve both obstruction and regurgitation. The valvular segments are shrunken and rendered rigid by calcareous deposit. You see, at once, that the orifice is contracted and patescent. I shall call your attention, presently, to the changes as regards the walls and cavities of the organ in this specimen, with reference especially to diagnostic symptoms. Here is another recent specimen in which the morbid appearances consist of a little atheromatous and calcareous deposit on and above the aortic segments. The orifice in this specimen is not contracted, and the valve is not incompetent to fulfil its functions.

The existence of aortic lesions is shown by the presence of either or both of the aortic murmurs, viz., the aortic direct and the aortic regurgitant murmurs. The aortic direct murmur has the same relation to the heart-sounds as the mitral regurgitant, that is, it occurs with the first sound of the heart; it is a systolic murmur. Now, how are these two murmurs to be discriminated? The mitral regurgitant, as we have seen, has its maximum of intensity at or near the apex of the heart; it is best transmitted in a lateral direction to the left of the heart, and it is not propagated into the carotids. On the other hand, the aortic direct murmur has its maximum of intensity at the base of the heart, usually, indeed, a little above the base, in the second intercostal space; it is better transmitted in an upward than in a downward direction, being louder at the top of the chest than over the heart's apex, very rarely loud enough to be heard over the left lateral surface of the chest, and it is usually heard in the neck over the carotids. These differential points will suffice for the discrimination in the great majority of cases. They must be distinctly understood, and so impressed on the memory as to be recalled without effort, in order to be readily available at the bedside; but the points are so few and simple, that this will require but a little effort on the part of the student.

An aortic direct murmur is almost invariably present if obstructive aortic lesions exist. Its absence, under these circumstances, arises from weakness of the left ventricle, and it is very rarely the case that the aortic direct current is too feeble for the production of a murmur. But lesions which are not obstructive, which merely occur on a roughened surface, and, as regards their immediate importance, are innocuous, also give rise to this murmur; and the murmur, in the latter case, may be not less, but even more intense, than in the cases in which the lesions are important. The intensity of the murmur, and its quality of softness or roughness, have no significance as regards the gravity of the lesions. I shall presently call your attention to means, other than the characters of the murmur, by which the gravity of existing lesions may be determined. Moreover, an aortic direct murmur, as we shall presently see, occurs without the existence of any lesions, being due to a morbid condition of the blood; and the means of determining whether the murmur be due to lesions or not, will hereafter be a point for consideration. Suffice it to repeat, that aortic lesions, whether more or less grave, are almost invariably represented by an aortic direct murmur, and hence, the absence of this murmur is, in general, adequate proof of the non-existence of lesions at this orifice.

Lesions at this orifice which involve insufficiency of the valve, usually give rise to the aortic regurgitant murmur. This murmur accompanies the second sound of the heart. It is, strictly speaking, the only truly diastolic murmur, and hence need never be confounded with the other murmurs. It may generally be heard above the base of the heart, in the second intercostal space, especially on the right side; but its maximum of intensity is lower than this, near the left margin of the sternum, on a level with the fourth rib. It is propagated downwards rather than upwards, and is not unfrequently heard as low as the ensiform cartilage; sometimes it is propagated as low as the umbilicus. Not unfrequently it is heard over the apex. The murmur is usually soft and low in pitch. This murmur denotes the fact of regurgitation, but its significance is limited to this fact; it does not afford information as to the amount of regurgitation, or, in other words, the extent to which the aortic segments are incompetent.

The aortic direct and aortic regurgitant murmur are frequently associated. These two murmurs, of course, succeed each other precisely like the two sounds of the heart; they have the same rhythm. There is some liability of confounding these murmurs, when associated, with exocardial murmurs, or friction sound. The means of making the discrimination will be considered in connexion with the latter.

So much for the signs of aortic lesions. Let us now direct our attention to the diagnostic symptoms. In order to understand them, we must take into view the effects of aortic lesions on the heart. These effects are illustrated in the morbid specimen before me. You perceive that in this specimen the left ventricle is hypertrophied and dilated. The first effect of aortic lesions, whether obstructive or regurgitant, or both combined, is upon the left ventricle. It becomes, first, hypertrophied, and afterwards dilated. Next, the left auricle undergoes enlargement, and at length, if life be sufficiently prolonged, the right side of the heart becomes enlarged; but the enlargement of the right side of the heart, as a rule, is less than in cases of lesions seated at the mitral orifice. You perceive that, in this specimen, the increased size of the heart is chiefly due to the enlargement of the left ventricle.

Palpitation is a more prominent symptom in cases of aortic, than in cases of mitral, lesions. The heart acts with greater power, owing to the fact that the hypertrophy which results from the lesions affects especially the left ventricle. The patient is much more apt to complain of a powerful beating of the heart, and the augmented power is more marked when the hand is placed over the præcordia.

Pain exists oftener in connexion with aortic than with mitral lesions. Why this is so I am not prepared to say, but clinical observation shows that it is so. In the cases in which that most painful affection, angina pectoris, occurs, the lesions are generally aortic.

Pain and palpitation occurs in connexion with either obstructive or regurgitant lesions. But the latter give rise to certain symptoms which are distinctive of aortic insufficiency. One of these symptoms consists in paroxysmal distress, with a sense of impending dissolution. The patient suddenly feels as if the the action of the heart was about to cease; the pulse is small, and perhaps irregular; there is an indefinite feeling of anguish. The paroxysms pass off after the lapse of a few moments, and they may recur after shorter or longer intervals. Sometimes they are provoked by an obvious cause which occasions excitement of the heart's action; but at other times they occur without any apparent cause. The paroxysms to which I now refer, so far as my observations go, belong especially, if not exclusively, to the history of cases of aortic insufficiency. They are characterized by all the phenomena of an attack of angina pectoris, save the acute pain which belongs to that affection. They involve a liability to sudden death. And I may state here, that aortic, much more than mitral lesions, are apt to prove suddenly fatal; indeed, it is chiefly in cases of aortic regurgitant lesions that there is much danger of sudden death. Let me endeavour to explain the paroxysms to which I have just referred, and the occurrence of death suddenly in certain cases.

The immediate effect of aortic regurgitation is accumulation of blood in the cavity of the left ventricle. This cavity is distended with blood in proportion to the amount of insufficiency of the aortic valve. It is this accumulation of blood which leads to the hypertrophy and dilatation of the left ventricle. Under these circumstances a sudden increase in the accumulation is liable to occur, from various causes which accelerate the circulation. The ventricle then is unusually distended, and, especially after it has been weakened by dilatation, the ventricular contractions overcome with difficulty the distension, The muscular walls of the ventricle are paralysed by the pressure of the blood within the cavity. If, at length, the ventricular contractions are sufficient to expel the unusual accumulation of blood, the immediate danger is removed; the distress and feeling of impending death which accompany the condition just described disappear. But if the contractions of the ventricle fail to relieve the over-accumulation, of course it rapidly increases from the constant flow of blood from the auricle to the ventricle, together with the aortic regurgitation; the walls of the ventricle become more and more enfeebled by distension with a corresponding rapidity, and death takes place from paralysis of the heart.

The pulse, in certain cases of aortic lesions, offers some distinctive features. If the lesions are obstructive, the pulse is weakened, but the weakness is much less marked than in cases of mitral lesions with considerable obstruction or regurgitation. In cases of aortic insufficiency, however, the pulse is peculiar. The artery appears to strike the finger with quickness and force, and to recede quickly. The pulse is said to be jerking or collapsing. Dr. Hope called it the "pulse of unfilled arteries." This kind of pulse, in some cases, is quite characteristic. It is evidently due to the fact that in aortic insufficiency the aortic direct current meets a regurgitant current; the two currents come into collision. Another symptom pertaining to the arteries is sometimes very striking and characteristic. The arteries near the surface—the carotids, the temporal arteries, the brachial, &c.,—present visible movements with each ventricular systole. These movements, in some cases, are remarkably conspicuous; the arteries, as Dr. Stokes remarks, seem like wriggling worms beneath the integument. Some time age a patient at the Island Hospital presented this symptom in such a notable degree that the face and neck would at once attract the attention of any observer. This symptom, when marked, is quite distinctive, so much so that aortic regurgitation may be predicted with much confidence; but the symptom is by no means marked in all cases of aortic regurgitation, and hence its absence is not proof that regurgitation does not exist.

Dyspnœa is less frequently marked in cases of aortic than in cases of mitral lesions. The distress arising from the cardiac affection has reference to disordered action of the heart, more than to interference with the pulmonary circulation and the functions of respiration. General dropsy is less apt to occur, owing to the fact that dilatation of the right side of the heart is of less frequent occurrence.

I have considered, gentlemen, the diagnosis of mitral and aortic lesions as they occur separately. But cases not unfrequent present themselves in which lesions exist at both orifices.

The combination of mitral and aortic lesions is shown by the association of the mitral and aortic murmurs. Does not this combination constitute an obstacle to the discrimination of the murmurs? Generally not. We may have a single mitral associated with one aortic, or the two mitrals with one aortic, and *vice versâ*, or, finally, all the four murmurs may be combined. The mitral direct murmur and the aortic regurgitant murmur have such distinctive characters, that each is at once recognised without difficulty. The only source of any difficulty is in ascertaining that the aortic direct and the mitral regurgitant murmurs co-exist. But in most cases it is not difficult to determine this point. These two murmurs occur in the same period of time as regards relation to the heart-sounds, that is, both are systolic murmurs. But frequently the murmurs differ in pitch and quality, so much that it is at once evident they are two distinct sounds; one may be rough and the other soft, or one may be high and the other low in pitch. If they do not thus differ, the fact of the existence of the two murmurs may generally be ascertained by finding at the base of the heart and at the apex, an intensity of murmur distinctly greater than between these two situations. Sometimes a murmur is distinct at the base and at the apex, but wanting at an intermediate situation. You apply the stethoscope in the second intercostal space, and you hear a murmur; next, you apply the stethoscope over the carotid, and you find the same murmur, *i.e.* a murmur having nearly or quite the same quality and pitch. You know, then, that there exists an aortic direct murmur. You then carry the stethoscope gradually downwards towards the apex, and this aortic direct murmur is found either to diminish in intensity or cease before you reach the apex. But when you place the stethoscope nearer to, or over the apex, you find a murmur more or less intense; you carry the stethoscope to the left of the apex and you find this murmur transmitted to greater or less extent over the lateral surface of the chest, and perhaps to the back. You are then satisfied of the existence of the two murmurs, even if it should happen that they are of the same pitch and quality.

These means of discrimination will prove sufficient in the majority of cases; but it must be confessed that cases do sometimes present themselves in which it is difficult to decide whether there are two murmurs or only one, and, if the latter, whether the murmur be mitral or aortic. The cases offering this difficulty are those in which a murmur is produced by conditions within the ventricle; conditions involving neither aortic contraction nor mitral regurgitation. Roughness of either the ventricular aspect of the mitral curtains, or the aortic segments, sometimes exists, giving rise to a murmur heard with about equal intensity at the base and apex and also over the body of the heart. By way of distinction from an aortic direct and a mitral regurgitant murmur, I am accustomed to call the murmur just described an *intra-ventricular* murmur.

As regards the symptoms belonging to cases in which mitral and aortic lesions exist together, it is sufficient to say that the phenomena distinctive of each of these classes of lesions are combined in various proportions.

A few words in conclusion, respecting murmurs produced by lesions seated at the orifices in the right side of the heart. Valvular lesions at the tricuspid and pulmonic orifices are rare, and when they exist they are generally congenital. They are accordingly oftener met with in children than in adults. They may give rise to murmurs corresponding to those which emanate from the mitral and aortic orifices. I am not aware of the occurrence of a murmur corresponding to the mitral direct murmur. I know not why a *tricuspid direct murmur* should not occur; but I have no practical knowledge of its occurrence. I have the same remarks to make of a *pulmonic regurgitant murmur.* I suspect it occurs, but I have never made it out clinically. A tricuspid regurgitant murmur, I am persuaded, is not of very unfrequent occurrence. The tricuspid valve in health protects against regurgitation less completely than the mitral valve; and when considerable dilatation of the right ventricle takes place, either with or without mitral or aortic lesions, the tricuspid regurgitation is often a consequence. This is shown by the occurrence, in certain cases, of a jugular pulse synchronously with the contraction of the right ventricle. The tricuspid regurgitant murmur, however, as I suspect, is generally considered a mitral murmur, and I have not made the discrimination in practice sufficiently to indicate, from my own experience, the differential points. These points are stated to be the following:—The maximum of the intensity of the murmur at or near the xiphoid cartilage, and the propagation of the murmur to the right, rather than to the left of the heart.

A pulmonic direct murmur from lesions of the pulmonic orifice may frequently, if not generally, be discriminated from an aortic direct murmur by the following points:—The maximum of the murmur is in the left second intercostal space, and the murmur is not transmitted to the carotids.

The symptoms pertaining to the pulmonic and tricuspid lesions are those which denote enlargement, especially of the right ventricle and auricle, viz. venous congestion, jugular pulse, and dropsy.

[Speaking of the application of *auscultation* to ascertain the amount of danger occasioned by valvular lesions, Dr. Flint observes:]

We compare, in the first place, the aortic and pulmonic sounds in order to determine whether the mitral lesions represented by mitral mumurs involve contraction or insufficiency of the mitral orifice, and if so, to what extent? An effect of mitral obstructive and regurgitant lesions is to diminish the intensity of the aortic second sound. This effect is due to the diminished quantity of blood sent from the left ventricle into the aorta with each ventricular systole. The quantity of blood is diminished in proportion to the amount of obstruction or regurgitation. This is sufficiently obvious, and in proportion as the quantity of blood sent into the aorta with each ventricular systole is diminished, the aortic second sound is weakened. Diminished intensity or weakness of the aortic second sound is then a sign that mitral lesions are either obstructive or regurgitant or both.

The weakness of the aortic second sound may render the pulmonic relatively the more intense, but this mitral disparity is increased by another effect of obstructive or regurgitant mitral lesions, viz. an increase or intensification of the pulmonic sound. How is this effect produced? Obstructive and regurgitant mitral lesions, as we have seen, lead first to enlargement of the left auricle, next to pulmonary congestion, and next to hypertrophy of the right ventricle. Now, when they have induced the latter effect, viz, hypertrophy of the right ventricle, the blood is propelled from this ventricular cavity into the pulmonic artery in larger quantity and with more force than in health. Moreover, owing to the distension of the left auricle there is an obstacle to the free passage of blood through the pulmonary circuit. The consequence is, the valves of the pulmonic artery are dilated with abnormal force; the force of the recoil is proportionately increased, and the pulmonic second sound is intensified.

Weakening of the aortic, and intensification of the pulmonic second sound, are thus effects of obstruction and regurgitation arising from mitral lesions. We can readily ascertain these effects by comparing, by means of auscultation, the aortic and the pulmonic sound. We listen to the second sound of the beat successively in the second intercostal spaces on the right and left side of the sternum, and we find the pulmonic sound to preponderate over the aortic in intensity, more or less, according to the diminished quantity of blood sent into the aortic, and the amount of hypertrophy of the right ventricle. The greater relative intensity of the pulmonic sound becomes a criterion of the amount of either obstruction or regurgitation, or of both, incidental to mitral lesions. For this truly beautiful application of auscultation we are indebted to Skoda, of Vienna.

A similar comparison of the aortic and pulmonic second sound is applicable, for the same end, in cases of aortic lesions. Suppose we find, on examining a patient, the aortic direct murmur. This murmur may be due to lesions which occasion more or less obstruction, or it may be due to lesions which are innocuous, or, again, as we shall presently see, it may exist without any lesions whatever. The characters of the murmur give us no definite information respecting the conditions on which it is dependent. An aortic regurgitant murmur shows regurgitation, but the murmur gives no definite information as to the amount of regurgitation; the regurgitant amount may be small or large. Now, it is desirable to know whether the lesions situated at this orifice are of a nature to impair, to a greater or less extent, the aortic valve. Does this valve fulfil its function, and, if not, is it damaged much or little? A comparison of the aortic and pulmonic second sound affords us information with respect to this point. If the valvular segments are not affected, the aortic sound is not weakened, but preserves its normal predominance over the pulmonic. But if, on the other hand, the segments are more or less damaged, the aortic sound is proportionately weakened, and the pulmonic sound becomes, relatively, more intense. The morbid disparity is due, in this case altogether to weakening of the aortic, and not to an intensification of the pulmonic sound, as in the case of mitral lesions. This is one useful application of the comparison of the two components of the second sound, and our cases at the hospital will also afford practical illustrations of which you will have the opportunity of availing yourselves.

We have thus far considered murmurs which represent organic lesions of the valves or orifices of the heart. But, as I have intimated already more than once, an endocardial murmur may exist without any organic lesions. An abnormal condition of the blood may give rise to a murmur, the valves, the orifices, and the heart, in every respect, being perfectly sound. Such a murmur is distinguished from the murmurs due to lesions of some kind, as an *inorganic murmur;* or, since an inorganic murmur depends on some blood-change generally connected with anæmia, it is often called an *anæmic murmur.* It is also called a *hæmic murmur.* You will, perhaps, imagine that the occurrence of murmur without lesions, must give rise to difficulty and confusion in discriminating an inorganic murmur from those which are due to lesions; the latter, by the way, are distinguished as *organic murmurs.* The discrimination involves less difficulty than you may imagine, and, practically, the occurrence of inorganic murmur occasions no serious inconvenience in diagnosis.

Let me proceed at once to give an account of murmurs which may be produced without lesions. A mitral direct murmur may occur without any *mitral* lesions. I am not aware that any one before me has pointed out this fact. I know it to be a fact. I have met with two examples of this murmur, well marked, in cases in which examinations after death showed the mitral valve and orifice to be entirely free from lesions. In both these cases, however, there were aortic lesions involving insufficiency, and dilatation of the left ventricle. So far as my experience goes, therefore, the mitral direct murmur may exist without *mitral* lesions, but not without aortic lesions. I was at first at a loss how to explain the fact just stated, but reflection on the subject has led to an explanation which is to my mind satisfactory. It is as follows :—Distension of the ventricles with liquid causes the valvular curtains to float towards the orifices and to come into apposition. In fact, experiment shows that the auricular valves may be closed by injecting a liquid into the ventricles from the auricles. This experiment has already been made before you by the professor of physiology. Now, in cases of aortic regurgitant lesions, the left ventricle is already filled with blood by the regurgitant current at the instant when the mitral direct current takes place; and as a consequence, the valvular curtains are already in apposition. Hence, the mitral direct current throws the curtains into vibration, and the murmur is produced. The mitral direct murmur, under these circumstances, and indeed generally, has a peculiar character, due, as I suppose, to the fact that it is produced by vibration of the valvular curtains, and resembling the sound caused by permitting the lips to be thrown into sonorous vibrations with the expired breath.

Let me here make an incidental remark with reference to the valvular element of the first sound of the heart. In speaking of the mechanism of the production of this element in a previous lecture, I was careful to say that it was due to the sudden *tension* of the auricular valves. I did not say the *expansion* of these valves. This element of the first sound is considerably weaker than the second sound of the heart. Whenever we eliminate from the first sound the element of impulsion, the second sound becomes the more intense of the two. The second sound is propagated further from the præcordia than the first sound, because the valvular element of the first sound alone is propagated even for a short distance ; the element of impulsion extends but little beyond the præcordia. These clinical facts appeared to me for some time quite inexplicable; but they are now to my mind explicable. The element of impulsion being due to the movements of the apex against the thoracic valves, is limited to the præcordia for the same reason that, as we shall hereafter see, a morbid friction-sound is thus limited : and the relative feebleness of the valvular element of the first sound, as compared with the second sound, is due to the fact that the mitral and tricuspid curtains are already floated out and in apposition when the ventricles contract. The blood in the ventricles, when the ventricular systole takes place, does not expand the curtains protecting the auricular curtains—they are already expanded—but they are simply made tense: hence, the sound produced is weaker than the second sound of the heart, notwithstanding the aortic and pulmonic segments are much smaller than the mitral and tricuspid curtains.

To return to the consideration of inorganic murmurs. A mitral regurgitant murmur, and an aortic rugurgitant murmur, are never inorganic; the occurrence of regurgitation at these orifices, of course, involves lesions which render the valves more or less insufficient. It is never a question when these murmurs are present, whether they are organic or inorganic. Thus the question, whether an existing murmur be organic or inorganic (exclusive of the mitral direct murmur), relates only to the aortic direct murmur. An aortic direct murmur may be either organic or inorganic. It is determinable in individual cases, whether this murmur be organic or inorganic? In general, this discrimination may be made; but it must be confessed that sometimes it is difficult, and even impossible. Does not the liability, in certain cases, to confound the two murmurs, impair the value of the organic murmur as a sign of aortic lesions? It does not; because, in the cases in which it is difficult or impossible to determine whether the murmur be organic or inorganic, it is of very little practical consequence to make the discrimination. Let me explain this statement:—

If an aortic direct murmur exist in a case in which the heart is found to be enlarged; if it be associated with an aortic regurgitant murmur; if the aortic second sound be impaired, the aortic direct murmur is in all probability organic—aortic lesions are present. But if the murmur exist in a case in which the heart is not enlarged; if an aortic regurgitant murmur be not present; and if the aortic second sound be not impaired, the murmur may be either organic or inorganic; and it is of little practical consequence to determine whether it be organic or inorganic, for, if it be organic, the lesions which give rise to it are innocuous. In other words, whenever the lesions giving rise to an aortic direct murmur are of immediate importance, the heart is more or less enlarged; or an aortic regurgitant murmur coexists; or the aortic second sound is more or less impaired.

In most cases, however, we are able to decide that an aortic direct murmur is inorganic. What are the circumstances which go to show that it is inorganic? I will enumerate the most important. And, first, I have already mentioned three important negative circumstances, viz., absence of cardiac enlargement, of an aortic regurgitant murmur, and of diminished intensity of the aortic second sound. These negative circumstances are to be considered as rendering it probable that the murmur is inorganic.

The existence of anæmia is another circumstance. Inorganic murmurs are generally associated with the obvious appearances of anæmia, viz., pallor of the prolabia and face. Anæmia, however, may exist without marked pallor, and, on the other hand, the murmur is found only in a certain proportion of the cases of well marked anæmia.

An inorganic aortic direct murmur is apt to be associated with an inorganic *pulmonic* direct murmur. We recognise the existence of a murmur in each vessel by disparity as regards pitch and quality. The coexistence of the two murmurs is a circumstance rendering it probable that the aortic direct murmur is inorganic; that both murmurs are due to the same blood-change. I may remark here that a pulmonic direct inorganic murmur not very unfrequently exists without an aortic direct inorganic murmur.

An inorganic murmur is generally soft; yet it is sometimes rough. Roughness, undoubtedly, renders it probable that the murmur is organic. A more important circumstance is the fluctuating character of the murmur. An inorganic murmur varies in intensity at different times, and may be sometimes wanting. An organic murmur is more uniform in intensity, and is almost always uniformly present.

In the cases in which an aortic direct murmur is inorganic, murmurs in the different arteries are generally found—in the carotids, the subclavian, &c. The same blood-change which occasions a murmur at the aortic orifice, gives rise to these arterial murmurs. It is generally easy to determine that the latter are produced within the arteries, not transmitted from the aortic orifice, by differences as regards pitch, and sometimes as regards quality.

Finally, certain cases in which an inorganic aortic direct murmur exists, are characterized by the coexistence of a murmur in the cervical veins. The murmur in the veins is a continuous humming sound, generally known as the *venous hum.* The French term it the *bruit de diable*, but by this title no disrespect is intended to his Satanic majesty. The devil in French is a child's toy—the humming-top—to the sound of which this murmur often bears a close resemblance. The murmur is sometimes loud and musical. It is heard on the right side of the neck oftener than on the left, and is frequently heard only when the patient is sitting or standing, not in the recumbent posture. For its production it is generally necessary that the stethoscope should be applied over the veins of the neck with a certain amount of pressure.

The venous hum was for some time supposed to emanate from the arteries of the neck. Some of the French observers still consider it an arterial murmur. That it is venous may be shown by a very simple procedure. In a case presenting the murmur, slight pressure above the situation where the stethoscope is placed, will cause an immediate suspension of the murmur, which reappears the instant the pressure is removed. It may thus be made to come and go by making and removing the pressure of the finger.

The venous hum is dependent on the same blood-change which occasions arterial murmurs, and an inorganic aortic direct murmur. It coexists often with a carotid murmur, and the latter continues when the venous hum is suspended by pressure above the stethoscope. Its coexistence with an aortic direct murmur is a point in evidence of the latter being inorganic.—*American Med. Times, Oct.* 25, *Nov.* 1, 8, 15, 1862, *pp.* 226, 239, 253, 268.

19.—ON RHEUMATIC PERICARDITIS.

By Dr. Henry Wm. Fuller, Physician to St. George's Hospital.

[Although in the judicious employment of alkalies in full doses, we have an efficient antidote to the rheumatic poison, and a sure protection against inflammation of the heart, yet cases constantly come under our care in which pericarditis has already commenced. A weakly girl, aged eighteen, was admitted with acute rheumatism of two week's duration, and extensive effusion in the pericardium, but no audible murmur. The points worth noticing in the case are best seen from the following remarks.]

This case affords a fair example of a severe attack of rheumatic pericarditis, and there are several points in relation to it to which I would direct your attention. In the first place there was an entire absence of exorcardial murmur at the date of the patient's admission into the hospital, but nevertheless the existence of pericarditis was diagnosed, and treatment was adopted accordingly. You must have observed the same fact in other instances, and it is of importance that you should remember that mere absence of friction-sounds affords very imperfect evidence of the non-existence of pericarditis. The sound of friction implies the rubbing together of the two surfaces of the pericardium roughened by the effusion of lymph, and, therefore, the presence of any large quantity of fluid in the pericardium necessarily puts a stop to the occurrence of friction by separating the two layers of the membrane. You will readily understand, then, inasmuch as in certain cases effusion into the sac is very abundant, and takes place with great rapidity, you may happen not to see the patient, or may not examine his heart until after the sound of friction has ceased, in consequence of the occurrence of effusion. But shortness of breath, or a complaint of pain in the chest made by a patient suffering from rheumatic fever, should always arouse your suspicions as to the existence of pericarditis, and so also should marked irregularity in the force and frequency of the pulse. Under these, and, indeed, under all circumstances, it is your duty to examine carefully into the condition of the heart, and, in the absence of pleuritic and pulmonary disease, the presence of extended præcordial dulness and a muffled condition of the heart's sound is quite sufficient to render imperative the adoption of means to get rid of the effusion. In the case under consideration, as the area of præcordial dulness decreased, and the heart's sounds became clearer, the sound of friction became audible; but I would warn you not to suppose that your diagnosis has been wrong, simply because from first to last you are unable to detect the presence of friction-sound. Extended præcordial dulness disappearing under treatment, and a muffling of the heart's sounds, which, after a time, is replaced by clearness of the sounds coincidently with the disappearance of the præcordial dulness, is quite sufficient to stamp the character of the disease. The fact that you have been unable to discover any friction-sound during the absorption of the effused fluid, may be due to the rapidity with which adhesion has taken place, and the shortness of the time, therefore, for which the friction has continued; or it may be referable to the non-effusion of lymph on the anterior surface of the heart, and to the consequent non-transmission of the friction-sound to the parietes of the chest. In the case of W. G., who was admitted into the Hope ward on the 3rd of April, there can be little doubt that this was the case; for although, as you will remember, he had all the general symptoms of pericarditis, and, with the exception of friction-sound, all the physical signs of the disease, and although under the influence of blisters, and calomel and opium, the symptoms all subsided, and the sounds of the heart became clear, yet from first to last no friction-sound was heard, although the heart was carefully examined daily.

The treatment of rheumatic pericarditis is a subject on which various opinions have been expressed, and I would therefore direct your attention to certain points in connexion with it. Theoretically, the objects to be attained are—1st, the removal of the cause of irritation which first excites and subsequently keeps up the pericardial inflammation; 2nd, the subjugation of the mischief which has been set up; 3rd, the mitigation of its evil effects by inducing absorption of the fluid, or other products which are effused into the pericardial sac. The first object is to be compassed by the administration of alkalies, which, as I have shown you, have the power of preventing the accession of cardiac inflammation, and which also, by eliminating the rheumatic poison from the blood, do much towards removing the articular, cardiac, and other inflammations which are due to the presence of that poison in the system. The second and third objects are to be secured by the use of bloodletting, counter-irritation, and the administration of calomel, opium, and other remedies. In the case of E. K., and in many other instances which have come under your observation in the wards, you have seen the plan of treatment I usually adopt. It is founded on the theoretical considerations just alluded to, and has proved eminently successful. The mixed alkalies are administered, calomel is pushed until the gums are slightly touched, and opium is given with it in quantity sufficient, not only to restrain its purgative action, but to subdue the irritability of the system. At the outset of the disease, if the heart's action is very turbulent, a few leeches may be applied to the præcordial region, or a few ounces of blood may be drawn from the arm; but in the great majority of cases depletion is unnecessary if the alkalies are properly administered, and in most instances it proves mischievous by depressing the patient, and thus preventing the setting up of those actions whereby absorption of the inflammatory products and adhesion of the two layers of the pericardial membrane are effected. Blisters are serviceable, even from the first, but their efficacy is displayed so much more strikingly when liquid effusion has taken place into the pericardium that I seldom have recourse to their assistance until the existence of effusion is unequivocally declared. In the case of E. K., a large amount of fluid existed in the pericardium at the date of her admission into the hospital, and the immediate relief afforded by the blister was manifest in the increased clearness of the sounds of the heart, and the occurrence of pericardial friction, which within three days resulted from the two roughened surfaces of the pericardial membrane having come into apposition in consequence of absorption of the effused fluid.

The importance of maintaining the strength of the patient whilst measures are being adopted for the relief of the local inflammation is well illustrated by the case of E. K. Up to a certain point all seemed going on well. The rheumatic pains subsided; the area of præcordial dulness gradually diminished; the heart's sounds became clearer; and, in short, all the symptoms betokened the disappearance of the rheumatism, and the absorption of the fluid products of the pericardial inflammation. Nevertheless, the friction sound continued, adhesion of the two layers of the inflamed membrane did not occur, and recovery did not take place. The explanation of this is to be found in the feeble condition of the patient. Naturally a weakly person, she had undergone two weeks of severe suffering before her admission to the hospital, and although the measures then adopted for her relief very speedily caused a mitigation of her pain, and relieved the heart of the large amount of fluid which was oppressing it, it was impossible at once to impart that tone to the system which is essential to the rapid organization of effused lymph. The feeble pulse, the alkalinity of the urine, which continued notwithstanding the omission of the alkalies, and the profuse and scarcely acid perspiration which ensued, were the exponents of the cause which prevented the organization of the effused lymph. The exhaustion which those symptoms indicated was the true cause of the non-cessation of friction, and is usually the origin of the untoward symptoms by which death in pericarditis is preceded. Accordingly, when these symptoms fully declared themselves, and it became evident that, notwithstanding the absorption of the more fluid parts of the exudation, adhesion would not take place between the two layers of the pericardium, I deemed it necessary to endeavour to support her, and thus enable the reparative process to take place. From the first I had given her strong beef-tea ; on the 27th I ordered her fish for dinner ; on January 3rd wine was prescribed ; and on the 5th I gave her the cinchona draught, although up to this time there had been no diminution in the sound of friction. Strange remedies these, you will say, to subdue an acute serous inflammation ! In truth they would be very strange remedies for such a purpose, but that was not the object for which they were administered. Blisters, and calomel and opium, and alkalies were the agents employed for that purpose ; the food, the stimulant, and the bark were only given to support the general strength, and enable these agents to do their work. And right well they effected the object in view. From the time the bark and wine were administered the symptoms rapidly abated ; the pulse regained its force and steadiness, the profuse, enfeebling perspirations ceased, and the friction sound diminished, and soon disappeared altogether. Here, then, is an important practical point for your consideration. None of the functions of the body, and none of the actions which are necessary to the reparation of the tissues, can go on properly in a person enfeebled beyond a certain point, and in the treatment of pericarditis, as of all other inflammatory diseases, it is essential to uphold the patient's strength directly you obtain evidence of constitutional exhaustion.

It has been stated that the administration of calomel is unnecessary in the treatment of rheumatic pericarditis, and that opium will do all that is needed for the patient's recovery. In proof of this, cases have been cited in which opium alone has been administered, and recovery has taken place. But I would urge you, gentlemen, not to be misled by such a specious argument. There is no drug and no method of treatment of which as much could not be said; nay, cases will sometimes recover without any treatment whatever, and there is no remedy, of the efficacy of which in the treatment of acute serous inflammation experience has afforded more convincing proof than it has of mercury. In pericarditis, in pleurisy, and in acute sclerotitis you must have noted the continuance of the symptoms until the effect of mercury on the system has been declared, and their immediate decrease and speedy cessation afterwards. The distrust which some persons have felt of mercury in these cases has arisen from their having improperly employed it. You must have seen patients suffering from pericarditis or acute pleurisy admitted into this hospital in a state of salivation, the friction-sound or the pleuritic effusion, as the case may have been, continuing nevertheless. And so it would have been in the case of E. K., if I had persisted in the administration of calomel without at the same time upholding her strength. Mercury is serviceable in proportion as the exudation is plastic and readily organizable; it is useless, or even mischievous, when the exudation consists of curdy, flocculent, ill-concocted lymph of low vitality. And as the former character of exudation occurs only when there is a fair amount of power in the system it follows that if the administration of mercury is pushed in weakly persons, or in persons whose kidneys are organically unsound, or even in stronger persons who are much exhausted by sleeplessness and suffering, or by unduly depressing treatment, the result must necessarily be unsatisfactory. The only chance of obtaining its remedial effects under such circumstances is by upholding the patient's strength during the period of its exhibition, as was done in the case of E. K. But that opium will check the continuance of the inflammation, and carry the patient safely through the attack, as is maintained by the opponents of mercury, is a fallacy which it requires little skill to expose. It will not prevent the accession of inflammation of the heart, as is proved by a series of cases published some time since by Dr. Sibson, in which a fearfully large proportion of the patients suffered from this complication of their rheumatism; it does not materially shorten the duration of rheumatism, or, in other words, does not check the action of the rheumatic poison, as is manifest from an inspection of the same series of cases, and of others which Dr. Corrigan has put on record; and inasmuch as the articular and cardiac inflammations arise from the same cause, it is obvious that it cannot control the one form of inflammation any more than it does the other. The truth appears to be, that in the one case as in the other, it calms the excitement of the nervous system which accompanies the disease, and thus economizes strength and enables the patient to endure his illness better than he could have done otherwise. Thus it is that when mercury has been improperly administered, and the patient is suffering from the effect of the irritation induced by salivation, the influence of opium in tranquillizing the system, and so promoting the actions which are necessary to recovery, is very striking. Little more is needed under these circumstances than to keep the patient under its influence. But it does not possess the power of preventing the accession of the disease, or of controlling its course, nor can it, like mercury, originate the actions which are necessary to the removal of the mischief. But, as already stated, it calms the excitement of the nervous system, procures sleep, economizes strength, and thus enables the patient to pass fairly through his trials; and when the disease has run its course, it still sustains and tranquillizes, and so obviates that extreme exhaustion which prevents the setting up of the actions which are necessary to the reparation of the mischief which has occurred. Therefore, whilst I would earnestly deprecate the practice of trusting exclusively to opium in pericarditis, and dissent theoretically and practically from those who urge its claims to the exclusion of mercury, I would have you understand that it is a valuable remedy, and should always be had recourse to in aid of those medicines which exert a distinctly curative power.—*Lancet, Dec.* 27, 1862, *p.* 695.

20.—ON PERICARDITIS CONSEQUENT ON PYÆMIA.

By Dr. W. S. Kirkes, Assistant Physician and Lecturer on Medicine at St. Bartholomew's Hospital.

The following communication consists almost *verbatim* of part of one of the Gulstonian Lectures delivered by me before the Royal College of Physicians in 1856, the substance of which lectures forms a portion of a work on "Diseases of the Heart," on which I have been for some time engaged. The publication of this paper at the present time is suggested by the occurrence lately at St. Bartholomew's Hospital of a case so exactly confirmatory of many of the views herein stated, that to those especially who observed the case the details now given may prove interesting.

The train of morbid phenomena developed by the introduction into the blood of the elements of pus or other like disintegrating and decomposing animal matter, though oftentimes obscure and not always constant, is still sufficiently well recognised and understood in the present day to need no special description here. It may, however, be observed, that when, after Surgical operations, wounds, or other local injuries followed by suppuration, the general venous blood is contaminated by the entrance of any noxious material from the affected part, the lungs are the organs which almost invariably suffer first and most. This happens, in great measure, probably, because they present the first set of capillaries through which the vitiated blood has to pass. Hence, in cases of ordinary pyæmia, however induced, the lungs are almost invariably, and sometimes exclusively, the seat of the so-called secondary deposits so common in this form of disease. When not described as existing in the lungs in such cases, it is probably because of their not having been sought for with sufficient care, or because of their having been overlooked, which may readily happen, if they be few in number and of small size, and not specially searched after; for the peculiar colour and texture of the lungs, and their frequent infiltration with blood and serum, in persons dying from almost any internal disease, often render the detection of morbid changes and secondary deposits far more difficult in them than in other organs, such as the liver, spleen, or kidneys, in which such deposits, when they occur, are usually quite obvious.

The changes produced in the lungs in such cases will probably vary somewhat according to the nature of the material whereby the blood has been vitiated. Without discussing these now, or entering at all into the deeply-interesting subject of pyæmia, I may only observe that, whatever be the contaminating matter introduced, many of the results, both in the lungs and elsewhere, are purely of a mechanical kind, and due to the arrest of the foreign particles in the capillaries traversed by the poisoned blood. Such mechanical arrest does not necessarily imply that the particles are too large to traverse the capillaries. Pus corpuscles, for example, are no larger than the white corpuscles of the blood, and, therefore, are not precluded by mere size from passing through vessels which the latter can traverse. Often, too, the foreign matter consists mainly of mere granular *débris*, resulting from the disintegration of pus or other cells, coagulated albumen, or fibrine, and, therefore, far too minute to be stopped even in the finest capillaries, unless under the influence of other than mechanical causes. Probably much of the impediment to the transit through the minute vessels of blood thus vitiated by morbid matters is due to the fact, that the new material introduced is foreign to the healthy composition of the blood, and that its presence interferes with that mutual relation between the blood within, and the tissues outside the vessels,—a relation apparently essential to the due maintenance of the capillary circulation,—and thus leads to retardation and ultimate stagnation of the contaminated blood. Be the real cause what it may, however, an impediment to the passage of blood through the pulmonary capillaries seems to be one of the earliest results of vitiation of the general venous blood by inflammatory or other like products. This impediment soon leads to complete stagnation, and then to coagulation of the arrested blood; then follows gradual decolorisation, and ultimately disintegration and softening of the coagulated blood. Around and within such foci of coagulated and decomposing blood, inflammation is excited, and this, with its products, contributes to the formation of the peculiar red, then brown or buff-coloured, and ultimately soft and yellowish masses which are often found so abundantly scattered through the lung-substance in these cases.

Sometimes the secondary mischief terminates at the lungs; the amount of contamination may not be great, and the patient recovers; or it may be very great, and the patient dies before any subsequent changes ensue, and the lungs are then found studded with these secondary formations. Usually, however, these formations in the lungs undergo various changes, resulting especially in softening, liquefaction, and decomposition. In this way they become productive both of further serious local mischief, and of dangerous contamination of the arterial blood. When the deposits occur near the surface of the lung, as they often do, they may, by their proximity to the pleura, excite inflammation of the portion of that membrane covering them. Such local inflammation of portions of pleura over secondary formations within the lungs is of very common occurrence, and is of the same nature as the pleurisy over tubercular or other deposits near the surface of the lungs. Sometimes the inflammation extends beyond the part immediately implicated, and may become more or less general. Sometimes, again, the portion of pleura covering one or more of these masses, softens, sloughs, and then the contents, which may have liquefied and suppurated, pass into the pleural cavity, and excite general and intense pleuritis. I have seen several instances of this kind; also more than one instance in which not only the softened matter from secondary formations, but also air, had passed into the pleural cavity, and thus given rise to pneumothorax, just as pneumothorax originates in the bursting of a tubercular vomica near the surface of a lung.

Besides exciting local mischief and leading to pleurisy and pneumothorax, deposits in the lungs may soften, break up, and be converted into reddish-brown or greyish pulp, or liquefy into a yellowish-white purulent material, and thus give the lung the appearance of being studded with so many abscesses.

Beyond the danger of such collections of matter bursting into the pleural cavity, there is the even greater danger of their contaminating afresh the blood in the pulmonary capillaries from which the pulmonary veins spring, and thus giving rise to wide-spread mischief on the distribution of the arterial blood thus vitiated to the various systemic parts of the body. Such a result not unfrequently ensues, and it is, probably, the chief cause of the various secondary inflammations and formations of pus found in and about joints, within muscles, the substance of internal organs, and other parts of the body, when the venous blood has been contaminated by phlebitis or any other like cause, and has led to the formation of pyæmic masses within the lungs. It is probably quite exceptional for purulent deposits to be found in various parts of the body after Surgical operations, or other such causes of vitiation to the venous blood, without the lungs having been first and principally affected. Part of the vitiated blood may sometimes pass through the pulmonary capillaries without its foreign constituents being all filtered out, and thus the systemic organs, as well as the lungs, be affected in the first instance; but it is probably by the softening of masses first occurring in the lungs that arterial blood is usually contaminated, and deposits elsewhere are induced. When arterial blood in thus contaminated at the lungs, all parts to which it is distributed are liable to suffer. Some parts, however, usually suffer more than others, partly, perhaps, from their receiving a larger and more direct supply of blood, though chiefly from causes which are as yet imperfectly understood.

Among other parts which may be the seat of such tertiary —if I may so call them—formations consequent on this vitiated state of the arterial blood, the muscular substance of the heart is occasionally found included. The occurrence of the deposits in this situation seems scarcely to have attracted the attention of pathologists, yet it is a circumstance involving features of grave interest and importance. Several cases of the kind have fallen under my notice. The following are the main points observed in one such instance :—

A lad, about 13 years old, struck his left knee violently against a piece of wood, and for several days suffered much pain in the part. Five days afterwards, general febrile symptoms, with pains in the limbs and joints, ensued; and after these had lasted three days, he was brought to St. Bartholomew's Hospital in an almost typhoid state, with sunken features, congested conjunctiva, dry lips coated with sordes, petechial spots over his arms, and a feeble pulse of 140. On auscultation a rough pericardial friction-sound was detected over the heart. Several joints were tender on pressure, and one knee swollen. The articular affection and the pericardial friction-sound were sufficient to justify the opinion that the lad was suffering from rheumatism, attended with cardiac complication, and to warrant the application of leeches over the heart, and the employment of calomel and salines at short intervals. After a bad night, during which he was rambling, moaning, and quite sleepless, the lad's aspect and condition next day were plainly indicative of rapid sinking, and in a few hours more he died. Examination after death disclosed much that was instructive. The left knee and thigh, which were the parts first injured by the blow, presented most extensive mischief. The periosteum of the femur was completely separated from the lower three-fourths of the bone, except along the linea aspera, by a large quantity of thick pale-brown, gruel-like material, which was also largely infiltrated into the soft parts of the thigh. The shaft of the denuded femur presented a smooth, healthy surface, and, on section, the bone was found quite natural. The knee-joint itself was healthy, as were also the other large joints examined, with the exception of the right elbow and left hip, both of which contained a turbid, oily fluid, instead of the proper synovia. Most of the veins proceeding from the neighbourhood of the left thigh, which was so extensively diseased, were more or less blocked up by old coagula; the deep femoral was almost completely obstructed by them. The femoral itself contained recent clots mingled with the old. From some of the divided veins small drops of yellowish puriform material, together with little pellets of old fibrine, escaped on pressure. The right femoral vein and its branches, and the iliacs on both sides, were healthy and free from old coagula. The inferior cava, however, just at the entrance of the common iliacs, contained, together with recent soft clot, a lump of old, brownish coagulum, freckled in places with whitish specks, and slightly adherent to the inner surface of the vein, which at that part was somewhat roughened and reddened. Along the remainder of its course the inferior cava was healthy, and contained recent clot.

The lungs, as might be expected from this extensive mischief in the thigh, and the evidence of much morbid matter having been introduced into the veins, were the seat of abundant secondary formations. These varied greatly in size, colour, and consistence: some as small as shot, others as large as hazel-nuts; some quite firm, and of a deep reddish-black colour, like masses of pulmonary apoplexy; others, in the opposite extreme, soft, pulpy, and semifluid, consisting of greyish or yellowish-white purulent material; while between these two extremes were all varieties, indicative of gradual decolorisation and breaking-up of previously dark red solid masses.

It may be observed here that secondary formations in the lungs, resulting from contaminated blood, very frequently present the same general characters as those occurring in this case. Occasionally, their resemblance to masses of ordinary pulmonary apoplexy, and the transformations which those masses undergo, is so close that they might readily be mistaken for them, were it not that the formations in question vary far more in size, and are more numerous, than those of pulmonary apoplexy, and that they are usually scattered through all parts of the lungs instead of being confined to the lower portions, as is generally the case with pulmonary apoplexy. Moreover, the absence of valvular disease of the heart, of general congestion of the lungs, and other frequent concomitants of pulmonary apoplexy, will usually serve to distinguish the two kinds of masses, should there be any doubt about them.

The pericardium was universally inflamed, its surface vascular and coated with lymph, and its cavity containing much turbid fluid. Beneath the lymph covering the surface of the heart, and underneath the pericardium, several small yellowish nodular elevations were found imbedded to some depth within the muscular substance of the heart. These masses, when cut into, were found softened in the centre. Further examination of the muscular tissue showed that similar yellowish masses were abundantly scattered through the substance of the left ventricle and of the septum, the right ventricle being entirely free. The masses varied in size, shape, and consistence; some as large as a split pea, and either embedded in the thickness of the walls, or projecting on the outer surface or the interior of the heart; some were tolerably compact and firm, others softened in the centre; while several were composed of a thin creamy material, which ran out when the masses were cut across, and thus left so many hollow spaces, like small, half-emptied cysts or abscesses. The number of these deposits was large, no section of the left ventricle being made without disclosing several; some looked like mere points or drops of pus: there was no undue vascularity around any of them. All the valves were healthy. The right cavities contained ordinary separated coagula, which presented the general characters of recent healthy clot.

There were several deposits in each kidney; some presenting the character of minute drops of pus surrounded by a zone of redness, others consisting of larger masses of mingled red and yellow, identical with what used to be described as patches of "capillary phlebitis." There were also several deposits of thin, milky-looking pus in the muscles of the right arm and forearm, and in other parts. These purulent masses, however, were quite different in character from the greyish-brown, gruel-like material about the left femur, and had, probably, a totally different origin, being, no doubt, true secondary, or rather tertiary formations.

Now, what were the most prominent features presented by this case? A severe blow over the knee, followed, after three days' suffering in the injured part, by general febrile symptoms, pains in various joints, petechial spots on the surface indicative of contaminated blood, inflammation of the pericardium, great prostration, and death in eight days after the receipt of the injury. After death, there was found extensive mischief about the part first injured—the periosteum separated from the bone, and the adjoining soft tissues infiltrated with exuded matter; old and degenerating coagula in the veins leading from the damaged part; abundant deposits in the lungs, many of which had softened down into abscesses; deposits of pus in the muscular tissue of the heart, and inflammation of the pericardium, without any attendant affection of the valves; and, lastly, deposits of pus in the kidneys, and in various parts of the muscular system.

What is the explanation of this chain of morbid phenomena? Clearly, the local injury was the first link in the series, for it was inflicted while the lad was in health, and it preceded by several days the manifestation of other symptoms. Extravasation of blood, with subsequent inflammation and suppuration between the periosteum and the bone, probably succeeded the blow, for the patient suffered much pain in the part for the next three days. The exuded matter and extravasated blood probably remained pent up for awhile beneath the periosteum, and from this source the venous blood became contaminated by the introduction of morbid material within it. The lungs being the organs through the capillaries of which the vitiated venous blood would have first to pass, the first and chief stress of the mischief would naturally fall upon them, and hence the abundance of the secondary masses they presented. The subsequent softening and liquefaction of these masses would tend to contaminate the blood brought by the pulmonary veins to the left side of the heart, and in this way the whole arterial current might be vitiated, and the deposits of pus in the muscular tissue of the heart, the kidneys, and elsewhere, be readily explained.

So far the interpretation of the several phenomena in the case seems quite obvious, for a closely-parallel series of events may not unfrequently be traced in other instances. But how are we to account for the inflammation of the pericardium? It was obviously not of rheumatic origin, for the pain and swelling of the joints was doubtless due to subsequent deposit of pus within and around them, and there was no rheumatic odour of the skin, and, what is perhaps more conclusive, there was no affection of the valves of the heart, for it is most rare to meet with rheumatic pericarditis independent of coincident disease of the valves. The correct interpretation probably is, that the pericarditis was caused by the deposits in the tissue of the heart, which, either by their mere proximity to the surface, or by actually bursting into the cavity of the pericardium, had excited inflammation of that membrane. The occurrence of partial or universal pericarditis from such a cause is rendered probable from the parallel occurrence, already alluded to, of partial or universal pleurisy in consequence of secondary deposits within the lungs.

I have met with several cases of pericarditis associated with the same chain of phenomena as observed in the instance just related; namely evidence of some primary local mischief in a bone or joint, followed by symptoms indicative of contaminated blood, deposits in the lungs, which had broken up and apparently contaminated the arterial blood, and subsequent deposits in various parts of the body, including the muscular substance of the heart. In some instances, the soft, pulpy state of the pericardium over portions of these deposits or small abscesses at the exterior of the heart, seemed to leave but little doubt that part of the contents of some of them had escaped, and thus almost necessarily excited inflammation of that membrane.

One such case, together with the recent instance at St. Bartholomew's, I may briefly introduce here, before offering the general remarks originally suggested by the case detailed in the former part of this paper.

A boy, about 17, was admitted into St. Bartholomew's Hospital under the late Mr. Stanley's care (July, 1854), and died in a few days with symptoms of pyæmia. Seven days before his death he was attacked, while in ordinary health, with pain in the right groin and thigh; this increased so much that next day he was unable to go to work. The pain continued without extending to other joints, and was speedily followed by febrile, then by typhoid symptoms, accompanied by petechial and pustular spots over the body. On the last day of his life the physical signs of pericarditis were detected. On autopsy, a large collection of greyish pus was found in the right hip joint, but there seemed to be no disease either of the articular cartilage or of the adjoining bone or periosteum. Within the lungs were abundant purulent deposits, many of them near the surface. Patches of recent pleurisy, with soft, friable lymph, were observed over these superficial deposits. The pericardial sac contained turbid fluid, while its two surfaces were roughened by recent lymph. At first sight no purulent deposit was observed in the muscular substance of the heart, but subsequent examination detected at least one small collection of pus near the external surface of the organ; probably closer examination would have revealed more. The valves of the heart were quite healthy. Several small deposits of pus were found in one kidney ; the other kidney, as well as the other viscera, were not examined. There was no affection of the right femoral, iliac, or the inferior cava vein.—*Medical Times and Gazette, Oct.* 25, 1862, *p.* 431.

21.—CASES SHOWING THE VALUE OF DIGITALIS IN ENFEEBLED HEART—CLINICAL REMARKS—DIGITALIS A CARDIAC TONIC.

Under the care of Dr. HANDFIELD JONES, St. Mary's Hospital.

Case 1.—A short, stout-made man, aged from 65 to 70, was admitted February 22, 1861. He had been ill several weeks, with short breath and pain in the front of his chest, gradually increasing. He is now in great distress for breath, panting quickly, and unable to get into an easy position. He moves about restlessly, cannot lie down, gets only a little broken rest, looks ill and worn. Legs dropsical, swollen, and firm. Skin cool. Pulse feeble. Heart's sounds weak ; no evident murmur; the area of dulness is greatly extended, both to the right and to the left. The impulse is very weak and diffused. No arcus senilis. Good breathing in both lungs, except at lower and posterior part of the right. Dr. Jones said : "I felt timid about giving digitalis, yet I thought no drug so likely to benefit the heart by its stimulant and tonic action, so, with some fear and trembling, I prescribed—

"Tr. ferri muriat., ♏x. ; tr. digitalis, ♏x. ; spt. æth. chlor., ♏x. ; aq., ℥j., ter die."

25th. To Dr. Jones' great surprise and satisfaction, he came from his room at the top of the house, down three pairs of stairs, to the ground floor. He seemed so much better that it was scarcely credible. His breath was still short, but he had none of that panting distress which had been so marked three days before. His legs were less swollen. His pulse, and the action of his heart, were much better, stronger, and firmer. He was still unable to lie down comfortably. He lay best on the left side. The heart's sounds were still free from murmur. A pill of cannabis indica and stramonium, which Dr. Jones had prescribed for him, had not appeared to be of much use, and was now changed for one of extract of hyoscyamus.

April 2nd. He has lately had a relapse, and is confined to his room with the same symptoms.

℞. Inf. digitalis, ℥iv. ; spt. æth. nitr., ʒiv; spt. æth. sulph. co., ʒjss. ; pot. nitratis, ʒj. ; aq. menthæ sativæ ad., ℥vi. ; m. cap., ℥j., ter die. Rep. pil.

6th. He is again much better.

13th. Going on exceedingly well; walking out, and seeming in fair health. Scarcely any swelling of legs. Pulse good, steady, regular, quiet. For the last few days he has had the same mixture, with sesquicarbonate of ammonia and tincture of digitalis, which he was taking a month ago, and also the iron and arsenical pills. The arsenic is now omitted, but the mixture continued twice a-day.

Some weeks after this he was anxious to do something for his maintenance, and took a place in his old vocation as a courier with a party, but he got no further than Cologne, where he was taken ill with his former symptoms, and died in the Hospital there.

Remarks by Dr. Handfield Jones.—Although other drugs were given in this instance, I have no doubt that the great improvement was chiefly owing to the digitalis. For—1. It was too rapid to be the effect of the tincture of the sesquichloride of iron; and the chloric ether cannot be considered as having any remarkable virtue. 2. When bark was substituted for the digitalis, a relapse occurred, although he was still taking ammonia and iron. I am certain, although the notes only afford negative evidence of it, that there was no considerable diuretic effect produced. Certainly, the rapid amelioration of the first few days was not owing to clearing away dropsical fluid. The heart was evidently hypertrophied and dilated, its valves presumably healthy, but its muscular tissue certainly enfeebled either by interstitial fibroid deposit (Jenner), or by fatty change. The urine, I think I can be sure, though I have not a special record of its state, was fairly healthy, not albuminous. The point of the case is, then, that digitalis was given in a condition when the heart's action was certainly failing, and the circulation becoming seriously embarrassed; and that, in place of enfeebling the cardiac action still more, it evidently reinforced and renewed its functional capacity for a considerable time.

Case 2.—E. G., a carpenter, aged fifty, was admitted February 27. He has been ill one year. He has had no rheumatic fever nor "inflammation of the chest;" no arcus senilis, and he is not anæmic. He complains that walking a little distance brings on a violent fluttering and trembling sensation at the præcordia, attended by a sense of suffocation, and also pain in the shoulders and numbness of the right hand and great perspiration When this goes off it leaves the "heart heavy," and he is excessively weak, and has a feeling as if his knees would give way, while, at the same time, he is continually making water, which is clear. He says he is often obliged to press his side to get relief from the heavy, dead, gnawing pain. A full meal will increase his distress; he cannot lie down for the sense of suffocation. Pulse weak, small, regular, visible at both elbows, scarcely so at wrist. Jugulars not distended. No dropsy. Respiration quiet. No morbid sounds in posterior parts of chest. Right lung descends far, pushing down liver; left seems of normal size. The impulse of the heart is diffused, visible in epigastrium, not in normal situation. Its rhythm is quite regular. To the left, outside of papillary vertical line, at the base of the left of the sternum, up to both clavicles, and at both sides of neck, and at each side of the epigastrium, a loud, single, prolonged, blowing murmur is heard, loudest at the sides of the epigastrium. No other sound is audible. Murmur at heart is nearly synchronous with the radial pulse, rather precedes it. Exercise makes the murmur louder.

From the date of his admission to April 21, he took iodide of potassium, compound iron pill, with opium, strychnia, ammonio-citrate of iron, and sesquicarbonate of ammonia, in infusion of calumba. The last was of material benefit for some while, but at the end of the time he was in much the same state as at first. He was then ordered, ℞. Tinc. digitalis, ♏xij.; tinc. ferri muriat., ♏xv.; spt. æth. chlor. ♏x.; aq., ℥j., ter die.

This he took with some phosphorated oil, after the first three weeks, till June 2. On this he improved very much, and described himself on May 19 as having less fluttering, trembling, and breathlessness, but that as soon as he got tired his heart became "heavy and dead." The digitalis was omitted, as it seemed to be causing some gastric irritation. He went on with sesquicarbonate of ammonia in infusion of chiretta, and with the phosphorated oil, to which was added, on June 5, a scruple of the saccharated carbonate of iron three times a-day. On the 19th his symptoms were worse, but the breath sound was still clear, especially at the back of the chest. The digitalis, iron, and chloric ether mixture was now resumed, the dose of the first being fifteen minims three times a-day.

June 26th. The patient remarks, "I think that medicine seems to agree with me." He could lie down now for three or four hours, which he was unable to do before. To take the mixture four times a-day.

He continued decidedly improved till July 7, when he complained of a painful sensation, like that resulting from a violent blow at the region of the heart, which seemed to be connected with an attack of catarrh. On July 10th the iron and chloric ether were exchanged for a mixture containing four grains of quinine and fifteen minims of tincture digitalis. This seemed to agree fairly well with him for fourteen days, after which Dr. Jones lost sight of him for nearly a month. When he saw him again dropsy had set in, and there was considerable gastric disorder. The dropsy increased, and he died towards the end of September. The urine on August 25th was fairly healthy, free from albumen. Dr. Jones pressed very much to be allowed a post-mortem, but was refused.

Remarks by Dr. Handfield Jones.—The foregoing case is an exquisite example of great dilatation and enfeeblement of the heart. The hypertrophy was much less than in the first case, and the dilatation was quite predominant. No better case could be imagined to test the influence of digitalis, and the result was certainly unequivocal. Instead of causing any further depression of the enfeebled organ, it decidedly increased its tone and power, and enabled it to carry on its function better than when any other remedy was given. If digitalis were in any degree a cardiac depressant, it would have extinguished the weak contractility of this poor fellow's heart, instead of arousing it. No special diuretic action ensued from its administration ; and, as there was no dropsy for some considerable time, the improvement of the heart's action cannot be referred to any relief of the circulation in this way. On the whole, the two cases related afford some evidence that digitalis is a *cardiac tonic*, and certainly prove that it may be given with advantage in cases of great cardiac debility. The observations of Winogradoff and of Traube, as to the influence of digitalis on the pressure in the arteries, go very much to confirm this view ; and Kulp's account of the action of digitalis in inflammatory disease, seems to prove that it induces arterial contraction, rendering the hands and face cold. Its action upon the uterus in menorrhagia, and on the arteries in hemorrhages, seems, also, to be of this kind. Diminishing the frequency and arrest of the heart's action depend, according to Traube, on the predominating excitement of the vagi, the regulating nerves of the heart ; while increased action depends on the sympathetic nerves being more strongly excited. He regards the action of digitalis as at first stimulating and afterwards depressing. I am inclined to question this view, as certainly in the above cases there was no evidence of its causing depression at any time. It seems to me more probable that, when digitalis induces dangerous symptoms, it is from its producing a tetanic state of the heart, *i.e.*, stimulating this hollow muscle so strongly to contract, that it does not duly relax and admit a supply of blood. In any fatal case of digitalis poisoning it would be most important to ascertain the exact state of the heart's cavities, whether they were contracted or relaxed. On the whole, I am much inclined to think that digitalis may be more safely used in asthenic conditions of the heart than in the opposite. What little experience I have had of its use in delirium tremens points in the same direction.

It seems to be injurious in sthenic, and beneficial in asthenic cases.

The phosphorated oil referred to in the second case is made with phosphorus and almond oil, five or ten grains to the ounce. Dose, five drops.—*Med. Times and Gazette, Dec.* 13, 1862, *p.* 629.

22.—ON THE ACTION OF DIGITALIS IN CARDIAC AFFECTIONS.

By Dr. Arthur Leared, London.

[Dr. Leared considers that Dr. Handfield Jones has omitted one mode in which digitalis may be supposed to act beneficially in diseases of the heart; a mode first pointed out by Dr. Corrigan.]

In disease of the aortic valve, increased cardiac action is compensatory, and the sedative effects of digitalis would be injurious; in other words, the efforts of the ventricle to make up by increased action for the effects of either regurgitation or obstruction at its outlet, connot be interfered with without injury. In mitral valve disease, on the contrary, the indication is to diminish the action of the ventricle so as to allow it to be filled as much as possible, in order that the blood may be sent into the aorta in a full stream, and, therefore, digitalis is in such a case most useful. This ingenious explanation has appeared to me well founded, and as such I have been in the habit of mentioning it in my lectures, when speaking of the uses of digitalis. I may add, that it accords clinically with my own observations. The diagnosis as to the particular valve diseased is often difficult, and sometimes impossible. But may not the very different statements as to the action of digitalis in diseases of the heart be largely due to an undiscriminating exhibition of this powerful drug in pathological conditions of the organ which are totally distinct?—*Med. Times and Gazette, Dec.* 20, 1862, *p.* 669.

23.—ON THE ACTION OF DIGITALIS.

By W. B. Kesteven, Esq., Upper Holloway, London.

Attention having recently been directed to the effects of digitalis, it may not be uninteresting or uninstructive to your readers to have their attention directed to the opinions expressed in Dr. Fuller's recent work on "Diseases of the Chest." Dr. Fuller is well known as a man of sound practical judgment, and one who does not express an opinion without having duly weighed all the facts on which it is based. It is urged by Dr.

Fuller, that the general notion of the danger attending the administration of digitalis in dilatation of the heart is incorrect, inconsistent with fact, and founded on the erroneous impression, that digitalis exercises a depressing influence over the action of the heart, and, therefore, leads to accumulation and coagulation of the blood in its cavities, if not to actual paralysis of its muscular structure. Dr. Fuller affirms, that digitalis stimulates the muscular fibres of the heart, and augments the contractility of the capillaries; that when it kills, it is not by paralysis, but by tonic contraction and spasm of the heart; that, such being the case, it is a valuable remedy in dilatation, and dangerous only when administered in hypertrophy. The grounds given by Dr. Fuller for these opinions are thus stated by the author (p. 592):—

"1st. During many years, I have observed that the cases of heart disease most benefited by digitalis have been those in which the heart has been weak and dilated, and the pulse feeble and irregular. In these the pulse has become stronger and steadier, and less frequent under its action.

"2nd. In the only cases in which I have known death to occur suddenly during the administration of digitalis, the heart has been hypertrophied and firmly contracted. This may have been a coincidence, but, viewed in connection with the results of experiments to which I shall presently refer, it is, at least, a suspicious fact.

"3rd. Dr. Dickenson has pointed out (Med. Chir. Trans., vol. xxxix.), and I have repeatedly verified his observation, that digitalis, if given in full doses, induces violent uterine contraction, and checks uterine hemorrhage; and, inasmuch as its action in staying menorrhagia and uterine hemorrhage is permanent, it seems fair to conclude that it gives tone to the capillaries, and increases their contractility.

"4th. This view is borne out by what I have long since observed relative to its action in arresting hæmoptysis, viz., that, whilst effecting the object required, it does not weaken but rather increases the force of the pulse, though it lessens its frequency.

"5th. When patients die of delirium tremens, the pulse is usually rapid and fluttering before death, and the heart is found weak, flaccid, and distended with blood afterwards. These are just the cases in which, on the commonly-received doctrines as to the action of digitalis, the drug ought necessarily to prove fatal, and yet modern experience has shown that in these cases it is tolerated, even in excessive doses. My impression is, that its remedial action in these cases depends on its stimulating the heart, subduing its irritability, and increasing the tonicity and contractility of the heart and capillaries, so that the brain is better supplied with blood, and the effusion of its more fluid parts, which gave rise to the "wet brains" of habitual drunkards, is avoided.

"6th. It has been proved by experiments on animals (Dr. H. Jones) that when death is induced by digitalis, the heart is not flaccid and distended with blood, as is commonly supposed, but, on the contrary, empty, contracted to the utmost, and in a state of tonic spasm. All these facts confirm my view as to the action of digitalis; and if it is correct, its practical importance in relation to the treatment of cardiac dilatation can hardly be over-estimated."

The preceding extract places before us clearly all that can be said on the action of digitalis, that, although long, it will not, I believe, be found too long to claim the careful attention of your readers. Having myself observed the clinical facts stated by Dr. Fuller, I would, with much deference, beg permission to endorse the practical views thus put forth on the therapeutic value of digitalis.—*Medical Times and Gazette, Jan.* 17, 1863, *p.* 73.

DISEASES OF THE ORGANS OF RESPIRATION.

24.—GENERAL REMARKS ON ACUTE PNEUMONIA.

By Dr. J. Hughes Bennett, Edinburgh.

Definition.—By acute pneumonia should be understood a disease consisting of exudation of the liquor sanguinis into the tissues and air-vesicles of the lung, producing by its coagulation, condensation of the organ. It is preceded by local and general changes, accompanied by various phenomena and physical signs; and runs its general course in various periods, according to the extent of the disease, and general vigour of the constitution.

No doubt the act of exudation is preceded by congestion in the lungs; and the idea for a long time existed that, if a large blood-letting was practised at an earlier part of the disease, it might be cut short or strangulated at the onset. But it is impossible to recognise with any certainty mere congestion of the lungs in the living body. The late Professor Alison maintained that an inflammatory was to be distinguished from other congestions by its *tendency* to exudation. But if, on bleeding largely in any given case, no exudation follows, how can it ever be known that the tendency was there? Whereas, when, as most frequently happens, exudation occurs, and continues to spread notwithstanding bleeding, it is then only we have any proof that such tendency existed; together with the certainty that, in all such cases, the remedy has failed to fulfil its supposed function. All cases of supposed strangulation of pneumonia are assumptions, which never can admit of proof. Whilst, therefore, all undoubted instances of this disease are characterised by the existence of exudation, we must regard that as its essential phenomenon. For what is further known on this point, with an account of the blood-letting controversy of 1857-8, I must refer to my Clinical Lectures on the Principles and Practice of Medicine, 3rd edit., p. 268, et seq.

Natural Progress.—From the numerous cases I have collected, as well as from those I have myself carefully watched, it seems to me that, when an acute pneumonia is limited to a fourth of one lung, recovery may occur in seven days. If half a lung be involved, recovery may occur in from ten to twelve days; if two-thirds of a lung, in from twelve to fourteen days; and if a whole lung or both lungs be involved at once, in from fourteen to twenty-one days. It will further be observed, however, that such recovery always bears reference to the amount of exhaustion and feebleness of the patients; such as are strong and vigorous rallying soonest, and passing through the disease and subsequent convalescence in the shortest time. A pneumonia at the apex also is more slowly removed than one at the base. For my own part, I do not believe in the possibility of cutting short a pneumonia by a large blood-letting and by antiphlogistics, as was formerly supposed; but of this I am certain, that such practice invariably tends to prolong the disease, and produces a lingering convalescence, even should a fatal termination be avoided.

Termination.—The natural tendency of an acute pneumonia is towards recovery. Since blood-letting and antiphlogistics have been abandoned, I have not met with a simple pneumonia, single or double, that has proved fatal. Indeed the deaths have been very few, and only in cases complicated with organic diseases of the brain, heart, or abdominal viscera. I have, however, heard of simple cases proving fatal, when there existed extreme exhaustion and starvation before they came under medical treatment. This result, when compared with the uniform mortality of one in every three cases during the reign of the past antiphlogistic system of treatment, is the most satisfactory proof of the modern advance in practical medicine.

Diagnosis.—The diagnostic symptoms of pneumonia in the majority of cases are clear, and capable of ready appreciation. Occasionally, however, there are difficulties presented by the following diseases; viz., phthisis pulmonalis, certain cases of pleurisy with effusion, cardiac disease with hydrothorax, chronic bronchitis with collapse of pulmonary tissue, thoracic aneurism, and cancer of lung. Whilst any of these morbid states may be mistaken for pneumonia, it may also happen that any of them may complicate a regular pneumonia. I can here only indicate these circumstances, as a description of the diagnostic characters of each would lead me too far. For the general purposes of this inquiry, I think the diagnosis may be safely left to the general tact and observation of the profession.

Pathology.—I consider acute pneumonia to be a true inflammation, dependent on irritation (generally reflex) acting on the texture of the lung, and, as a result, producing congestion of the capillaries, terminating in exudation. This exudation of the liquor sanguinis insinuates itself into the air-vesicles and all the interstices between the ultimate tissues of the organ, and, subsequently coagulating, produces its condensation or so-called hepatisation. This, at first reddish or sanguinolent, becomes afterwards gray or suppurative; a change resulting from the exudation, as a molecular blastema, passing into pus-cells. Numerous researches have satisfied me that the cell-formation is caused first by molecular aggregation and formation of nuclei; and, secondly, by the deposition of cell-walls around them. In no instance, although frequently assisted by the pupils of Professor Virchow, have I ever been able to see any appearance capable of supporting the hypothesis put forth by that pathologist, which attributes the origin of pus in pneumonia to proliferation either in the epithelial cells lining the air-vesicles, or in the nuclei of the pulmonary fibrous tissues *(Verbindungs-körperchen).* I consider that, so far from being an exceptional and fatal occurrence, suppuration is the ordinary method by which nature breaks up the solid exudation, and renders it capable of rapid disintegration and absorption. In chronic cases where this is not accomplished, it remains molecular or granular. If this view be correct, all those circumstances which favour suppuration, such as local warmth, and maintaining the strength of the economy, are those which best favour the onward progress and rapid recovery from pneumonia; a result I consider established by the facts and statistics of my practice previously referred to.—*British Medical Journal, Oct.* 25, 1862, *p.* 436.

25.—SUGGESTIONS FOR FACILITATING THE USE OF THE LARYNGOSCOPE.

By Dr. George Johnson, Professor of Medicine in King's College; Physician to King's College Hospital.

[The following observations are made with a view to render the use of the laryngoscope more general and more successful.]

1. *On the position of the Concave Reflector.*—Hitherto, in whatever way the reflector has been supported, whether between the teeth, or in a spectacle frame, or on a band passing round the head, the practice, so far as I know, has always been to retain the reflector in front of one eye—usually the right eye—and the observer has looked into the throat, either with the one eye applied to the central opening in the reflector, or he has used only the uncovered eye; or he may, perhaps, have succeeded in using both eyes. Every manipulator, however, must have experienced a degree of discomfort and inconvenience, resulting from one eye being kept behind the reflector, and from the effort required to keep the eye adjusted to the aperture; more especially is this found to be inconvenient and troublesome when both hands have to be used in the performance of any operation on the throat. The discomfort and inconvenience here referred to are completely obviated by the plan, which I have recently adopted, of having the reflector so attached to the frontal band, that it shall come to the middle of the forehead, and leave both eyes uncovered. I am not aware that there is the least advantage to be gained by peering through a hole in the reflector. On the other hand, the advantages resulting from having the reflector in the position here indicated are that the manipulator, having the free and unrestrained use of both eyes, finds it much easier to direct the light into the patient's throat, to keep the faucial mirror in the required position; and, in short, to do everything for the performance of which two eyes are more helpful than one. The reflector can be fixed in the position by shortening the hook which attaches it to the frontal band. This can be done with the greatest ease. I made the alteration in my own instrument with a file and a pair of pincers in about five minutes. With the reflector on the forehead there is no need for the central hole. The manufacture of the instrument, therefore, may be more simple, and in the same degree less costly.

2. *A Darkened Room is not Necessary for Satisfactory Laryngoscopic Examination.*—I am in the habit of examining patients in the wards of the hospital, with only such a moderate exclusion of light as is to be obtained by drawing down the blinds of one or two of the nearest windows. The illumination of the throat certainly is easier and better with a good light in a dark room, but, since a darkened room is not always to be had, it is important to be able to make a satisfactory examination of the larynx in a room which is not completely darkened. All observers agree that the light of the sun, when it can be obtained, is the best means of illuminating the throat.

3. I have found, as others must have done, that the concave reflector is invaluable as a means of illuminating the throat, for

the purpose of examining the tonsils, palate, and pharynx. Placing a candle or a lamp on a table by the side of the patient, the operator, with the reflector on his forehead, throws the light into the throat, and has both his hands free to do whatever requires to be done—to depress the tongue, to apply caustic or other local remedies, &c.

4th. Since the preceding was written, I have devised a simple and very satisfactory method of autolaryngoscopy.—Sitting at a table of a convenient height, I place a common dressing-table looking-glass at a distnce of about eighteen inches in front of me, and a moderator or gas lamp at one side of the glass, but three or four inches further back, so that no light may pass directly from the lamp to the mirror. Now, with the concave reflector on my forehead, as before described, I throw the light from the lamp, as it were, into the open mouth of my own image in the looking-glass ; then introducing the faucial mirror, I at once see the reflection of my larynx in the glass before me, and any one looking over either of my shoulders can see the image at the same time. This method, therefore, serves for autolaryngoscopy and for demonstration : in plain English, the experimentor can thus see his own larynx and show it to others ; and the method here described possesses some advantages over that employed with such wonderful success by my friend, Professor Czermak.

I find that, while holding the faucial mirror with my right hand, and changing the position of my head and neck so as to obtain different views of the larynx, I can keep the light directed where it is required by adjusting the frontal reflector with my left hand. This cannot readily be done with Professor Czermak's apparatus, on account of the distance at which the reflector is fixed.—*Medical Times and Gazette, Feb.* 14, 1863, *p.* 157.

DISEASES OF THE ORGANS OF DIGESTION.

26.—ON THE TREATMENT OF TÆNIA BY THE OIL OF MALE-FERN.

By Dr. John W. Ogle, Assistant Physician to, and Lecturer on Medical Pathology, at St. George's Hospital.

[Dr. Pereira, in his well known work on Materia Medica and Therapeutics, observes of the use of the oil of male-fern in the treatment of tænia, "I have tried it in several cases of tapeworm, but without effect." A fair medium dose for an adult is about a drachm and a half. Dr. Ogle in the first part of this article gives a table of twenty-four cases in which the oil of male-fern had been employed, and then remarks :—]

On reviewing and considering the above table of cases, it will be at once obvious that the oil of male-fern was, at any rate, most useful in causing the death and expulsion of long lengths of the worm. In many cases the patient had not measured or estimated its length; but it will be seen that in one instance the worm expelled had been twenty-four yards long (Case 2), and in another no less than thirty yards long (Case 3). In a few instances, the existence of the worm had been known for a number of years, as for nine years (Case 11), for ten years (Case 1), &c.; but, in the average number of cases, they had been experienced from between one and two years. The ages of the patients will be seen to vary from 3½ to 57 years.

I regret much that my information is so deficient as to whether or not the root of the worm—the anchoring part, as Dr. Gull aptly terms it—that commonly termed the head—had been passed. In two cases it is most probable that the head or root was passed. The extremely small size of this, the attached extremity of the worm, the carelessness and inefficiency with which it is of course often sought for, the scanty accommodation enjoyed by the poor, all are circumstances which may fairly be supposed likely to obscure our attempts at determining whether this part has been at any time voided along with the general mass of the worm. I can entertain no doubt that the fine and comparatively pointed termination of the tænia is often got rid of—the entire worm, in fact, passed—without the fact being absolutely established.

As respects the action of the oil of male fern, the table, I think, shows pretty well what may be expected of it. Out of the whole of the twenty-four cases in which it was given, in only two cases was there any doubt as regards its poisonous and expellent action on the intestinal worm; and in one (Case 8), seeing that eight yards had been passed but a short time previously, it is not unlikely that there was no worm to be expelled.

As regards the period at which, after the male-fern oil had been swallowed, evacuation of the worm was produced, it appears that out of the twenty-four cases (excluding the two cases 4 and 8, before denominated as being doubtful as regards the action of the male-fern, and one case, 18, in which no information whatever upon this point was gathered from the patient), in twelve cases the worm was expelled within a short time after taking the remedy, and before the castor oil or other subsequent dose was given; in one case (13), it brought away the worm in one hour and a half after having been swallowed; in another case (18), in two hours afterwards; in another case (23), in three hours.

Touching the action of the remedy upon the patient, irrespective of its vermicide and vermifuge properties, it will be seen that, in by far the majority of instances this action was not in any degree or manner troublesome or disagreeable. In Case 10, the patient, aged 21 years, was described as having been rendered "very ill" by the medicine; in Case 11, as being made "ill all over," "weak, and trembling;" in Case 23, a "good deal of pain" was complained of when the worm was voided; and in Case 16, pain was experienced in the bowels about an hour after the male-fern had been taken. But, in by far the majority of the cases, it is expressly stated that no discomfort resulted from the medicine; and in none other were any complaints of it made. In Case 11, that wherein so many unpleasant effects were experienced during the action of the drug, it is noticeable that the worm was only expelled after four doses had been taken.

Cases 1, 10, and 12 would appear to show as decided a superiority in the vermicide action of this medicine, compared with turpentine, as it is superior on the score of taste and agreement, &c.

Such are the results which may be elicited by a consideration of the table, regarding the utility of the oil of male-fern in the treatment of tænia. I hope to continue to give this remedy in such cases; taking care, if possible, to learn all particulars regarding the mode of life, kind of food used by patients, and specially with reference to the eating of raw meat, ham, bacon, sausages, &c. I shall also take the precaution, which I would recommend to others (following the custom adopted with the nurses at Guy's Hospital), of promising a premium upon every "root" or head of the worm brought to me by the patient, so as the better to secure them; and, when possible, to admit the patients as in-door patients for a day or two, for the purposes of observation.

I will now pass on to make one or two observations which my notebook suggests respecting the uses of other remedies in tapeworm. Thus, I find, amidst a great number of other cases of which I have record (but so imperfect in various ways that I could not adduce them along with those placed in the table), that the oil of male-fern has certainly acted vigorously and abundantly in the destruction and expulsion of long lengths of the tænia when the kamæla (written also kamayla, kamala, and kamela) has failed. The form of kamæla which I used was the tincture, two drachms being the medium dose for an adult. In one case—a child, aged 4 years—after in vain trying the tincture of kamæla, forty drops of the male-fern oil, in mucilage, syrup, and mint-water, caused the expulsion of ten yards of the worm within three or four hours after the medicine had been administered.

I have also given the powder of the rottleria tinctoria, in about two-drachm doses, mixed with honey or mucilage, with the effect of expelling lengths of the tænia. In this dose it has been longer, for the most part, before producing its specific action, than the male-fern oil. In one case in which I gave the rottleria tinctoria, it is not unworthy of remark that the patient complained that, when the tænia caused intestinal irritation, in addition to "mistiness in eye-sight," a gnawing pain at the upper part of the abdomen and pain in the forehead, he very frequently experienced "red blotches," to use his own expression, on the forehead and nose. In this case, I find from my notes that no sickness or uneasiness ensued on taking the rottleria tinctoria powder. In other cases, also, this drug has been noticed as acting without griping or discomfort of any kind.

Again, I have given the kousso in ample (half-ounce) doses, but with variable (in some cases quite unsatisfactory) results; also the santonine, with, as yet, negative results. But I have reason to think that my doses of the latter drug have been too small for the purpose. I propose trying it in larger quantities. I believe it may be safely given in ten or fifteen grain doses.

On one occasion, I found that a two-drachm dose of the extract of taraxacum brought away a large number of the tænia joints, the presence of which within the intestines had been unsuspected. It is possible that this vegetable may be rendered available, in suitable doses and form, for the expulsion of intestinal worms; to which, indeed, all "bitters" appear to be inimical. In another case, a tapeworm was expelled after a large quantity of very strong coffee had been drunk.

There is another remedy of which I am about to try the effects, in consequence of its strong recommendation by Friedrich of Heidelberg; and that is the picronitrate of potash (*Kali picronitricum*).

I may remark, that it would appear that in certain cases there is an attempt on the part of the bowel to dislodge the worm, apart from medicine exhibited. Thus I know of one, a private case, in which for many years the patient has had the tapeworm, and who, every few months, experiences violent griping, with nausea (never vomiting) and diarrhœa, for about half a day, with concomitant expulsion of several yards of the worm. He has never found the root or head of the animal; but he does not appear ever to have sought for it very systematically. In another case of which I have notes, the patient observed that, contrary to what is usually thought to be the case, he had had three worms of large size expelled within a short period of each other, in each of which the head was found. This observation, though stated honestly by the patient, may, of course, have been erroneous.

More than once I have found that the tænia and the ascaris lumbricoides have existed in the human bowel at the same time.

With one more remark I will conclude, and it is one which may possibly have a practically beneficial effect; and that is, that I think there may be reason for believing that the tænia may be communicated to infants and children from others, and in the following manner. In almost all cases, there is reason to think that a tænia does not exist very long within the intestine without portions or joints being passed, not only by stool—*i.e.*, with the fæces—but also by their spontaneously working their way through the anus, and "skedaddling" (to use an expressive Scotch phrase) from the person, adhering to the garments, or falling on the ground. Now, I have known cases in which there is reason to think that these fragments, which soon desiccate, shrivel, and become reduced to a small brownish-coloured mass, may have been picked up by children, often crawling infants, who are ever ready to pounce upon and put in their mouths every crumb or crumb-like substance which they can see, and thus have been introduced into the digestive organs. I have no certainty of this; but I think it is a point worthy of consideration and attention, and one to be mentioned to parents in cases where the tænia is known to exist in any members of a family.—*British Medical Journal, March* 14, 1863, *p.* 266.

27.—OIL OF MALE-FERN IN TAPEWORM.

By Dr. Alexander Fleming, Birmingham.

I agree with many in believing that the oil of male-fern is a a cure for tapeworm; but I often meet with medical men who deny its efficiency. Now, an appeal to the wide field of observation which our Association offers will surely decide this question. I have found, moreover, that the oil sometime fails; and I trust our inquiry will disclose the causes of failure and enable us to avoid them.

Care should be taken that the drug employed be fresh and of good quality. It is sometimes not so. It is a thick dark olive-green coloured oil, having the characteristic odour and taste of the male-fern root. The taste is cooling and persistent. I have not perceived the acridity that is commonly ascribed to it. In default of better tests, we judge of the quality by the colour, taste, and smell. The inferior qualities may be due to the roots of other ferns being gathered in place of that of the male-fern; or, more probably, to unskilful preparation and preservation. The best quality, if kept for some time, throws down a grey sediment. The virtue of the oil does not appear, so far as I can learn, to be thereby injured; but, when a deposit has formed, the bottle should be well shaken, before pouring out the dose.

The mode of exhibition is important. I usually give it thus:—Mix well oil of male-fern one drachm, and mucilage half an ounce. This draught is mixed with one ounce and a half of sweet milk, and taken at bedtime, the patient having omitted the dinner and evening meal of that day. Taken thus, on an empty stomach, the mixture is carried speedily into the intestines, to feed, and at the same time poison, the hungry parasite which nestles there. Milk is the favourite food of the worm. If the bowels incline to constipation, I give next morning a dose of castor oil; otherwise not. If necessary, I repeat this medication daily, one, two, and three times, or until the worm is discharged. A large experience has satisfied me of the decided efficacy of this mode of exhibition; but the remedy may be given successfully in other ways, and each observer will naturally incline to adopt that mode, the value of which has been confirmed by his own trials.—*British Med. Journal, Nov.* 22, 1862, *p.* 529.

28.—A CASE OF TÆNIA TREATED BY THE ARECA NUT.

By Dr. Edwin Morris, Spalding.

My attention was first called to the treatment of tænia by the areca nut, by an able article from the pen of Dr. Barclay, of Leicester, inserted in the British Medical Journal, August, 1861. Dr. Barclay says: "The areca nut is the kernel of the fruit of the species of palm called *areca catechu*, it is commonly called the betel nut, being used along with the leaves of the betel pepper for chewing in eastern countries. It requires to be ground and then pounded. The dose I give is usually four, five, or six drachms, which is easily taken stirred up in milk. The worm is usually discharged four or five hours afterwards alive. The taste of it is simply astringent; but with all remedies I have found the necessity for a smart purgative the day previous to their administration, and for a fast the evening before the vermifuge is taken.

Hitherto, all remedies administered for the expulsion of tapeworm have been so disagreeable to take, or uncertain in their effects, as to make any medicinal substance more palatable to the patient and more certain in its action, very desirable; and when I first read Dr. Barclay's paper I was so struck with the simplicity of the remedy as well as with its cheapness, that I readily availed myself of the following case for its administration.

John T., aged forty-six years, for several weeks past had been losing flesh, had occasional headaches, nausea, and feeling of faintness, with loss of appetite. The month of April last, he had passed several pieces of tapeworm. After fasting from breakfast the previous day, on April 30th, at six a.m., four drachms of powdered areca nut was taken in milk. Within two hours afterwards he passed six yards and a half of worm. For more than a week afterwards, no more worm coming away, four drachms more of the areca were taken in a similar manner. Within an hour six yards more of worm were passed. From the flat regular size of the worm, it was evident that the head had not yet been expelled. After previously fasting, a purgative was given; and early the next morning six drachms of the areca were given as before, and in a quarter of an hour two yards and a half more worm were passed alive, making altogether fifteen yards. Upon examining the worm, it gradually tapered down to a small bulb about the size of a pin's head. The man has been much better since, and no more joints have been passed.

I was fortunate in having obtained so long a piece of the neck of the worm, as it enabled me to easily find the head when placed in water; having removed the head with a portion of the neck attached, and placed it under the microscope with a one-inch power, I readily examined it as a transparent object. The neck is narrow and flat, terminated by a small bulb (cephalic bulb) with black specks, which is the head of the worm; upon carefully examining the specks, they are found to be the four suckers, and are funnel-shaped and easily recognised. The necks of the two extreme suckers ramify in various directions, and appear to join the other suckers. Upon using a higher power (Smith's and Beck's fifth) several hooklets may be seen, by which the worm attaches itself to the mucous membrane of the bowel before applying its suckers. I have made a sketch of the head as it appears under the microscope, which gives a tolerably accurate representation of the head with its four suckers and hooklets.—*British Med. Journal, Oct.* 18, 1862, *p.* 413.

29.—ON SUBCUTANEOUS INJECTION OF MORPHIA IN CHOLERA.

By Dr. Isaac Ashe, Birkenhead.

[The following cases occurred in the practice of Dr. Ricketts, of Birkenhead, who, perceiving the value of opium in cholera, and also the difficulty of administering it either by the stomach or rectum, without its being immediately rejected conceived the idea of employing it as related in sequence, and with success in several instances.]

Case 1.—Mrs. H., aged thirty-five, on November 11th was seized about 7 o'clock a.m., with violent purging and vomiting, occurring about every quarter of an hour, and attended with severe cramps in the bowels, extending down the limbs. The evacuations were of the usual rice-water character; coldness and collapse came on very speedily; and when Dr. Ricketts was sent for at 10 o'clock, or three hours after the commencement of the attack, the patient was already cold and livid in countenance.

Dr. Ricketts immediately injected ♏xv. of liq. morph. acet. beneath the skin of the abdomen. In quarter of an hour the cramps were completely removed, and the patient expressed herself as very comfortable. Dr. Ricketts administered, also, a few grains of calomel. From the moment of injection there was no return of either purging or vomiting; collapse was removed, warmth returned to the surface, and she rapidly regained strength. The next day she was able to be up, and felt almost as well as ever, being able even to go about and attend to household matters.

Case 2.—At half-past 12 o'clock the same night, Dr. Ricketts requested me to see a patient, with similar symptoms. He was an old Indian veteran, aged sixty-four, and had often seen men struck down in India with cholera, and was, consequently, very well aware what was the matter with him. I found him suffering from violent purging, which had occurred, he estimated, about twenty times within the eight hours which had elapsed from the commencement of the attack; vomiting had come on about two hours later, and was continuing with great frequency and severity. The cramps in the bowels were so severe, that the stout old soldier was crying out with them; they extended also down the thighs; collapse had not yet commenced, but the surface was much colder than normal, though the pulse was 104. Following Dr. Ricketts' treatment, I injected ♏xv. of liq. morph. acet. beneath the skin of the abdomen, and, wishing thoroughly to test the efficacy of this plan of treatment, determined to have recourse to no other measures. In a quarter of an hour the cramps were quite gone, and the patient exclaimed about the wonderful powers of the remedy. They did not again recur, though the purging returned, but with less frequency; vomiting also recurred once or twice during the night. Accordingly, some hours afterwards, Dr. Ricketts injected a second ♏xv. of the liq., which had the effect of completely arresting all purging and vomiting. The patient was up the next day, complaining only of a little weakness, and on the following day he set out on a journey.—*Med. Times and Gazette, Dec.* 13, 1862, *p.* 644.

30.—PODOPHYLLIN IN JAUNDICE.

By Dr. **George Harley**, University College, London.

[Dr. Harley, in an article "on the action of mercurials, benzoic acid, and podophyllin, in jaundice," says:]

Podophyllin or May-apple, the American remedy, is said to possess both the alterative and the purgative effects of mercury. As an alterative, it is given in doses varying from one-eighth to one-fourth of a grain three times a day; and as a purgative, from one-fourth to one grain as a single dose. Its purgative action is rather slow in manifesting itself, and therefore it is best given overnight; and, should it fail to produce its effects within a couple of hours after rising, the dose ought to be repeated, and its action courted by means of warm drinks. There is one disadvantage connected with this drug; namely, that in nervous females it occasionally gives rise to disagreeable griping.—*British Med. Journal, Dec.* 20, 1862, *p.* 635.

URINARY ORGANS.

31.—A QUESTION AS TO PATHOLOGICAL DISTINCTIONS IN CASES OF DIABETES.

By Dr. **Daniel Noble**, Manchester.

[Whilst diabetes mellitus has always been regarded as a formidable malady, what is known as diabetes insipidus has been considered a comparatively unimportant ailment; thus the whole stress as to the importance or non-importance of the case being made to depend on the presence or absence of sugar in the urine. Dr. Noble has recently been led to think, however, that the detection of sugar in the urine does not necessarily give the very grave character to a case, as is generally supposed, and, in fact, the presence of sugar need not be regarded as the sure precursor of that particular diabetic condition which almost invariably has a fatal termination. The writer illustrates this view by first two cases of a well-known class, and then by two others the pathological signification of which is essentially different.]

A young Greek, about seventeen years of age, was brought to me some years ago, by the proprietor of a boarding-school in the neighbourhood of Alderly. The complaint was that the patient had become gradually listless and weak, incompetent alike for physical exertion and mental application; and that this state of things had, for a few weeks, been almost imperceptibly making its way. On inquiry, I ascertained the existence of thirst, voracious appetite, dry skin, and large discharges of urine—symptoms which, of course, suggested the diagnosis of diabetes. I directed that the young man should not return to school, but should remain with relatives who resided in the immediate neighbourhood, intimating at the same time to the gentleman who accompanied him, that I regarded his case as very serious, and that he ought to go to bed and have constant and systematic attendance; that, if my own services were further demanded, I must be informed, and, in that case, should visit him on the following day, and would wish to see the quantity of urine voided after a given hour. It was soon afterwards intimated that I was to render him such attendance and aid as the circumstances might require, and I called upon him accordingly on the following day. I found the poor youth much weaker than on the previous day even, and ascertained that he had parted with immense quantities of urine possessing an odour and other characteristics rendering the presence of sugar very probable. I took some of it away with me, and submitted it to simple tests; it may suffice to say that the urinometer manifested a very high specific gravity, and Trommer's copper test exhibited a very large proportion of sugar. On the succeeding morning, the debility and exhaustion had very alarmingly progressed, and I gave the worst prognosis, stating that it would be agreeable to myself if they should wish to call in some other assistance—a suggestion which, it was said, should be considered. I paid an evening visit on the same day, and was told, without seeing the patient, that in consequence of my opinion expressed in the morning, I had been superseded; that my successor, moreover, should be replaced in like manner, if he did not speedily effect an improvement; and that, in fact, doctor after doctor should be tried upon this principle. Early the following morning, however, the patient died.

Now, in this instance, there was obviously a collapse of so speedy and decided a character, that little can be said of its specific nature; the peculiar perversion of function indicative of diabetes mellitus so rapidly carried with it a destruction of all vital energy as to preclude the attempt at any special ascription of its origin and cause.

In the course of last year, a lady consulted me on account of her daughter, who for some weeks had been losing flesh and strength; she was not more than about thirteen years of age, and had been under the care of a well-known homœpathic practitioner in this city, who did not appear to have made any definite diagnosis. On directing my attention to the young lady, my first impression was that she was labouring under some obscure form of phthisis, but a physical examination of the chest and a quiet pulse, excluded the idea; I next thought, on learning that her appetite was unusually good, of possible mesenteric disease, but I could obtain no corroboration of this notion on extending the inquiry; it was when excessive thirst was mentioned, that I thought of diabetes. The skin was dry, the appetite voracious, and the urine extraordinarily abundant; I had brought to me the same day a specimen of the urine, and found it of high specific gravity, and containing large proportions of sugar; the nature of the case was thus put beyond doubt. For about a week I saw this patient on alternate days at my own house, and she was seen once also by Sir James Bardsley. After some seeming improvement, which did not continue for more than a day or two, she began rapidly to decline, and was unable to leave her room; therefore I attended her at home, and in three days she expired—ten days from the period when she was first brought to me.

In this case, the invasion of the malady could be traced to no particular cause; it appeared that the decline in strength had shown itself slowly and insidiously; and it was only the persistence and augmentation of the debility with notable emaciation, that led the parents to regard the illness in any serious light; for, indeed, they had been encouraged by the homœopath, they said, to feel every confidence that time and his own treatment would bring about recovery. This instance, like the preceding one, constitutes a fair illustration of a class of cases familiar to practitioners and dependent upon conditions of the system but little understood,—cases which alone, probably, at no very remote date, would have had their diabetic character made out, and the general symptoms of which would alone have suggested the importance of testing the urine for sugar in order to settle the diagnosis.

But there is another class, which I proceed very briefly to exemplify. A gentleman between fifty and sixty years of age, of great mental endowment, one who had through life pursued a thoughtful and laborious career, had, for upwards of twenty years, been accustomed to consult me when requiring medical aid; his ailments having generally been some feverish cold, or a slight degree of gastro-enteric irritation that rendered his digestion habitually weak. For four or five years prior to the summer of 1860, he had been deeply engaged in labours demanding much brain-work, of a kind, moreover, that involved considerable worry and anxiety. About the period just specified, he became troubled with neuralgic pains about the head, with broken sleep and gastric irritation to an unwonted extent. I was constantly urging detachment from business-cares and labours, but this recommendation was but imperfectly carried out; yet, to the extent to which it was so, the symptoms were always relieved. Late in the autumn the ailment assumed severer features; for great debility and some emaciation ensued.

[Having to visit London he consulted an eminent Physician there, who pronounced the case to be one of diabetes, put him on a regular plan of diet, medicine, &c., secundum artem. Dr. Noble certainly found a small amount of sugar in the urine, but that liquid did not in other particulars resemble diabetic urine, it was high-coloured and deposited lithates on cooling—the diabetic treatment had soon to be given up, and Dr. Noble persuaded him to rely entirely on detachment from harrassing occupation and thoughts, change of air, and recreative amusement, with such medicines and diet as his own particular experience might suggest to be the best calculated to restore the integrity of his digestive functions. Dr. Noble justly regarded the case as one of those in which mental irritation, by its action on the brain, gives rise to saccharine urine; this phenomema not being of that momentous and fatal significance which the popular estimation of diabetes would lead to think. The result justified this opinion, for recovery though delayed by one or two circumstances, was ultimately perfect.]

A gentleman of my acquaintance, of high intellectual endowment, and, moreover, of great sensitiveness of disposition, has for the last seven or eight years had saccharine urine—a symptom which ensued upon much anxious mental effort. As he is a resident at a considerable distance, I cannot exactly speak of this gentleman as my patient, although on several occasions I have had conversations with him on the subject of this particular derangement of his health. I think it was about the year 1854 or 1855 that sugar was first discovered in the urine, but he had for many years had an imperfect digestion, evidenced very often by lithic deposits, and he had been a bad sleeper. In this case, there has been none of that steady progress in a downward direction so commonly happening when there is saccharine urine. From the time of its detection to the present there have been several attacks of illness, and on two occasions the patient had been all but given over. I saw him about three years ago labouring under irritable heart as the chief malady; he was in bed and much exhausted, so much so, indeed, that his friends, having regard to the diabetes, scarcely expected that he would rally; yet, at this time the specific gravity of the urine was not high, below 1,030 I think. The gloomy forebodings were not realised, and recovery took place, enabling him to fulfil important and responsible duties. In this instance, there was, for the most part, a somewhat inordinate appetite, but not any particular dryness of skin nor any great excess of urine. I select for illustration a case, with which I am but imperfectly acquainted however, mainly because the connexion in degree between mental wear and tear and saccharine urine was on several occasions quite notable; for example, about four months ago, the patient was abroad accompanied by an intimate friend, from whom I had shortly afterwards the information that, during this absence from home, circumstances occurred to produce great stress of mind, breaking down the health for a brief period; that, apparently in consequence, the diabetic symptoms became very much aggravated, the specific gravity of the urine running up to 1,051 from 1,025. On the enjoyment of quiet and the restoration of mental calm, the *status quo* was very speedily recovered; and, since this occurrence, the patient has been better than for a long time before. Indeed, a gentleman well acquainted with him, writing about a month ago to me on another subject, refers to our common friend as follows: "He keeps well; I suppose there never was known such a case."

I think there can be no doubt that the class of facts, very imperfectly exemplified by the preceding accounts, demand the inference that some causal relation subsists between certain pathological states of the brain and the excretion of sugar by the kidneys; showing, as it would appear, that mental perturbation is one of the first links in the chain of diabetic phenomena, and, showing, moreover, that the degree of mischief exhibits some correspondence with the intensity of the presumed cause. Indeed, so obvious is the relation in question, that it has been suggested by some, that diabetes mellitus is essentially of encephalic origin; and, certainly, if pathological distinctions cannot be established, there is much evidence that may be cited in maintenance of some such thesis. The well-known experiments of Claude Bernard, repeated and confirmed by Dr. Pavy in this country, furnished no little plausibility to the doctrine; they shew that saccharine urine can be artificially produced by mechanical irritation of the floor of the fourth cerebral ventricle. And other experimental lesions of the nervous system, moreover, have been found to produce diabetic symptoms since the one first practised by M. Bernard. Ordinary brain-disease will sometimes develope for a time a saccharine condition of the urine.—*British Medical Journal, Jan.* 17, 1863, *p.* 59.

32.—EXAMINATION OF THE URINE IN DISEASE.

M. Bouchardat has arranged the necessary agents for such an examination, so as to make the latter easy. The principal agents employed are heat, nitric acid, tannin, lime, and an ioduretted solution of the iodide of potassium. 1. *Heat.* A little above 100°; the urine becoming turbid, will separate albumen in the form of clots or flakes. But it must not be forgotten that every specimen of urine which becomes turbid, under the agency of heat, is not necessarily albuminous; hence we must employ, 2. *Nitric Acid.* Add some of this acid carefully to the urine. If albumen be present flakes will be deposited. An excess of acid will dissolve the flakes. Any urine that becomes turbid under the influence of heat, and renders a precipitate under the action of nitric acid, contains, beyond all doubt, albumen. 3. *Tannin.* A solution is made by adding two hundred *grammes* of water to ten *grammes* of tannin, and ten *grammes* of ether are then added to preserve the solution. The action of this reagent cannot be relied upon, since the solution of tannin will produce an abundant precipitate, if soup, rich in gelatine, have been taken shortly before the analysis. 4. *Lime.* This detects admirably the sugar of diabetes. Fifty *grammes* of urine, and two *grammes* of lime, being boiled together in an assay bottle, will give the colour of caramel, more or less dark, according to the quantity of sugar present. The quick-lime is slaked with water, and then introduced into a flask which has a tight cork. If fifty *grammes* of urine are not coloured when boiled with two *grammes* of lime, then two *grammes* more should be added. After the mixture is boiled again, if no colour appear, it is well to test the lime. For this purpose, a half teaspoonful of starch-sugar is poured into the flask. On boiling, if the urine should then be freely coloured, we have proof that the lime had been properly calcined, and that the urine contained no sugar independent of the starch-sugar which had been added. 5. *Ioduretted Solution of Iodide of Potassium.* This is prepared by dissolving one part of iodine and one part of iodide of potassium in fifty parts of water. This solution will communicate a chesnut-brown colour to urine containing sulphate of quinine or any other alkaloid likely to be administered in therapeutics.—Jour. de Chim. Med. and Amer. Med. Monthly.—*Dublin Medical Press, Feb.* 18, 1863, *p.* 163.

33.—ON ASSIMILATION AND THE INFLUENCE OF ITS DEFECTS ON THE URINE.

By Dr. F. W. Pavy, Assistant Physician and Lecturer on Physiology at Guy's Hospital.

The term "assimilation" is limited by some to the process by which the constituents of the blood are transformed into the several components of the animal fabric; but I comprehend it as implying a more extended signification—the act of bringing to a likeness, whether of food into blood, or of blood into organs and textures. I follow Dr. Prout in this general acceptation that I am giving to the term assimilation. I follow him also in his subdivision of the term into primary and secondary assimilation. At the outset, however, I have to state that it is only with the former these lectures will have to deal.

Researches in organic chemistry have shown that the proximate principles of our food, vegetable as well as animal, present the same chemical characters and constitution as the proximate principles belonging to the solids and fluids of our bodies. Now, the object I have in view is, to take the chief alimentary principles and trace them through the process of elaboration—primary assimilation—which renders them fitted for administering to the requirements of life; and at the same time to show that, when these principles are allowed to reach the circulation without having first been properly assimilated, they prove useless to the system, and are ejected with the urine in the form they were introduced.

To give an illustration: the albumen of the egg, when received in the ordinary way of food, is applied in the processes of life, and lost sight of after introduction into the system. This same albumen of the egg, however, reaching the circulation without having undergone the proper assimilation is eliminated with the urine without having been changed or appropriated. Thus an unnatural condition of the urine is created, through an alimentary principle being permitted to enter the blood in its original form.

This experiment affords a striking proof—a proof that may be taken as ground to work upon—that something is needed to adapt our food for entrance into the circulation, in order that it may be retained in the system and made subservient to the economy of life. This something constitutes primary assimilation, which, as I have said, I propose to speak of as far as the information I possess will permit. By afterwards looking at the effect that occurs, where no assimilation has been allowed to take place, we shall see how conditions of the urine identical with those belonging to certain well known affections of the human subject may be artificially brought about.

In order to proceed systematically, I shall adopt the classification that divides our alimentary principles into albuminous, oleaginous, and saccharine groups. And I propose to take these groups in the order in which they have been mentioned.

First, then, of the albuminous group, which includes the nitrogenized constituents of our food—the plastic elements of nutrition of Liebig—the materials which go towards the renovation of our tissues. In this group we have albumen, fibrine, and caseine—principles procurable alike from both kingdoms of nature; and connected with these is animal gelatine, which is also an azotized alimentary principle. None of these principles are acted upon chemically till they reach the stomach. In the mouth assistance is given towards the subsequent carrying out of their assimilation; but this is only by a mechanical process of reduction. Through mastication our solid food is placed in a more favourable position for being penetrated and dissolved by the digestive fluid with which it is afterwards brought in contact. I have experimented with saliva, and have failed to observe it possess any digestive properties upon the albuminous group. At the end of six hours, in an experiment not long since performed, the hind legs of a frog, introduced into an ounce of saliva acidified with twenty minims of hydrochloric acid, and exposed to a temperature of 112°, remained without any appearance of digestion. It appears possible, however, that insalivation may aid gastric digestion by entangling air with the food; for during my experiments upon artificial gastric digestion I have certainly noticed that parts near the surface of the liquid which was exposed to the air have been more energetically attacked than those below.

The stomach is the portion of the alimentary tract which is specially intended for acting upon the albuminous group. It is through the medium of the stomach that the nitrogenized constituents of our food are liquefied, and thus reduced to a state fit for absorption. Such, in fact, is what is accomplished by the process that is spoken of as gastric digestion; and just, and only just, at the period required, a fluid is poured out from the glandular structures in the walls of the organ, which fluid constitutes the effective agent for attaining the object that is required.

In the position I am occupying it would, I consider, be a mistaken course if I attempted to waste the time of my audience by entering into full particulars regarding what occurs in the stomach. Nevertheless, there are a few points connected with the physiology of digestion as one of the steps towards assimilation, that I venture to think may be advantageously touched upon here.

It may be taken for granted as a sufficiently established fact that the solvent properties of the gastric juice over albuminous materials depend upon the presence of two principles—an acid, and a nitrogenized substance of ferment, known by the name of pepsine. It does not appear to signify what may happen to be the nature of the acid present, but we must have an acid of some sort with pepsine, as essential constituents of a digestive fluid.

I wish to give you visible evidence of the energetic properties possessed by the gastric juice, and to show you that neither the acid nor pepsine alone is capable of exercising any solvent effect. The test that I have been accustomed to employ in my experiments consists of the flesh of the frog. I have selected the frog for this purpose because its skin and flesh are easy of digestion, and because the effect is so neatly discernible. The animal is killed and the hind quarters are removed, just as is done for galvanoscopic purposes. A piece of thread being attached to the spine, the legs are suspended to about the middle of the thighs in the liquid to be examined, contained in a suitably-shaped glass or a wide-mouthed bottle. Exposure to a moderately elevated temperature is now necessary, and this I accomplish by carrying on the experiment in a water-oven with a thermometer introduced, so that the temperature of the interior may be with facility read off. The temperature I employ is from 100° to 110°. If below this, digestion is comparatively slow; and if much above it, digestion is prevented by the destruction of the nitrogenized digestive principle; but it requires about 140° for this to take place.

In this experiment (to which I will first allude) we have contained in the bottle an infusion of the dried mucous membrane of the pig's stomach treated with twenty minims of strong hydrochloric acid to the fluid ounce. This is the strength of acidity that I always employ. After exposure for four hours to a temperature of 110°, the effect has been, as may be preceived, that the portion of the frog's legs immersed in the fluid has been almost entirely dissolved. One leg appears as though amputated at the surface of the fluid, and the other has the foot removed, and the bones that remain denuded. There are some fragments of bones and shreds lying at the bottom of the vessel; but these in a short time longer would have completely disappeared. It is truly remarkable the solvent power possessed by this gastric juice.

In these two other experiments we have a simple infusion of the stomach on the one hand, and a mixture of hydrochloric acid and water, twenty minims to the ounce, on the other. Although there has been an exposure for upwards of six hours to the elevated temperature, yet not the slightest evidence of any solvent action is perceptible. The cutaneous surface remains quite as intact as when the experiments were commenced.

The nitrogenized principle necessary to the constitution of a digestive fluid is only yielded by the walls of the stomach. Nothing can be extracted from the mucous membrane of the intestine, bladder, or any other structure, that will give us a fluid endowed with the solvent properties enjoyed by gastric juice. I have here an infusion of the mucous membrane of the small intestine of a dog. It has been treated to the usual extent with hydrochloric acid, and exposed after the introduction of the frog's legs into it to an elevated temperature for between seven and eight hours. No signs of digestion are to be perceived.

It is affirmed, however, that the pancreas contains and yields a principle which is capable of exercising the same digestive properties over nitrogenized materials as the gastric juice. Purkinje and Pappenheim in 1836 obtained from an acidulated infusion of the tissue of the pancreas a liquid, which, according to their experiments upon albuminous materials, they considered to constitute a very active artificial digestive juice. Lucien Corvisart considers the pancreas as a kind of supplementary organ to the stomach, intended to digest, after a copious meal, what has remained undigested by the stomach. He says that the nature of the two digestions is alike so far as the result is concerned, each leading to the production of albuminose or peptone. Whilst, however, acidity is a necessary condition of the gastric juice, Corvisart states that the pancreatic juice enjoys the privilege of acting equally well, whether in an alkaline, neutral, or acid state. I have submitted the pancreatic juice to the test of the frog's legs, but have failed to find it possess an action that can, so far as I can see, be compared to that of the gastric juice.

In one experiment I took an ounce of good pancreatic juice—a juice that acted energetically in emulsifying fatty matter and converting starch into sugar—and treated it with thirty minims of hydrochloric acid. I used a larger quantity of acid than in my experiments upon gastric digestion, because the pancreatic juice possesses a strongly alkaline character which has first to be overcome. On the addition of the acid there was a copious effervescence, and a production of a white albuminous-looking precipitate. The frog's legs were introduced, but, after eight hours' exposure to a temperature of 112°, there was no appearance of digestion perceptible.

In the other experiment, the juice was taken in its natural state. The frog's legs were introduced into an ounce of it, and exposed for eight hours to a temperature of 112°. At the end of this time, there was a most offensive smell of decomposition given off, which is the reverse of what occurs with the employment of gastric juice. The soft structures of the legs were in a slimy state, and hanging loosely from the bones, which were exposed. The legs looked as though they had been immersed in water for some time, and were in an advanced state of putrefaction. The bones, however, were not in the slightest degree softened, and the joints were firm, the ligamentous structures remaining quite intact. With gastric digestion the bones and joints are attacked equally as well as the other structures. In fact, the effect produced by the pancreatic juice seemed more of a putrefactive character than a digestion comparable to what occurs under the influence of the gastric juice.

What is required, then, for that special digestion or solution of albuminous materials which is effected in the stomach, is a certain animal principle yielded only by the gastric mucous membrane, and the presence of an acid. As to the mode of action of the gastric juice, it is customary to say that it exerts its influence through the agency of catalysis or fermentation. Terms like these are easily employed; but they do not help us far towards elucidating the exact nature of the cause which leads to the manifestation of the peculiar efficacy enjoyed by the pepsine and acid in combination.

In conformity with the perfection that is throughout living nature noticeable in the adaptation of the means employed to the ends to be attained, the production of a digestive fluid only occurs at the period when its presence is required. It is only upon the application of the stimulus of solid extraneous matter, alimentary or otherwise, introduced into the interior of the stomach that the secretion of gastric juice takes place. When a meal has been disposed of, and the organ has become empty, its surface is only moistened with a glairy mucus, which may be neutral or even alkaline in character. In Dr. Beaumont's observations upon Alexis St. Martin, the secretion of gastric juice, as witnessed through the fistulous opening, was seen to take place from the surface of the stomach when excited into action, like the exudation of fine drops of perspiration upon the surface of the skin, and to distil from the mucous coat as a pure, limpid, colourless, and always distinctly acid, fluid.

Although I have no reason to take exception to what has just been stated, yet there is one point in reference to the secretion of gastric juice upon which I would wish for a short time to engage your attention, as I believe it to have a not unimportant bearing connected with it.

From what I shall have to mention, it would seem that the secretion of gastric juice must not be looked upon in so simple a light as it might appear susceptible. It is true the secretion of a digestive fluid is intermittent, but it is not both the essential constituents of this digestive fluid that are thus only intermittingly produced. The presence of pepsine is constant; whilst it is the acid fluid which distils from the mucous structure when excited by the irritation of foreign matter against the surface. Pepsine, in fact, exists in the mucus and in the epithelial layer under all conditions of the organ. Probably, therefore, the pepsine and acid are derived from different sources. From the mode in which the acid fluid appears, it would seem to come from the gastric follicles. The pepsine, on the other hand, seems to be derived from the epithelial layer forming the free surface of the interior of the organ. The mucus secreted by these epithelial cells has a special character in containing pepsine; and when the acid fluid rises from the follicles beneath, under the influence of the stimulus of food, it will necessarily mingle with the product of secretion of the epithelial surface, and so produce a digestive fluid. For reasons that I shall have afterwards to advert to, there seems to be a cause for rendering it probable that the acid and pepsine should not be secreted together by the glandular structures—the gastric follicles—situated in the substance of the membrane : the integrity of the organ itself might be seriously endangered thereby.

The pepsine that I have recently been employing in my experiments has been derived from the stomach at a period of fasting. To procure this principle, indeed, for the production of an artificial digestive fluid, it does not in the slightest degree signify whether the stomach be taken from an animal in which digestion is proceeding or not. It is from the stomach of the pig that I generally obtain my pepsine; and the pigs that yielded it for my recent experiments, I was informed, had not been fed since they left Holyhead on their way from Ireland to London, at which latter place they were killed. They must, therefore, have been fasting for a considerable time prior to death.

But to prove that pepsine is present in the stomach even after prolonged fasting, the following experiment was carried out under my own observation. A dog having been fed on the Saturday, was kept without food till the Tuesday following, when it was killed. Seventy-two hours had thus elapsed since the last meal. The stomach was removed and dried, and I have here an artificial digestive fluid that has been prepared from it. As may be perceived by the effect upon the frog's legs, it possesses the most energetic digestive properties. Before the stomach was dried, the viscid mucus that was in its interior was collected by gently scraping it with a blunt instrument from off the surface. This, when acidulated, presented a digestive fluid as active as that procured from the stomach itself.

The epithelial lining of the stomach, then, possesses the power of producing a mucous fluid which is peculiar in containing a principle that is not be met with elsewhere. This peculiarity in secretive action exists also as a constant character. At a period of repose as well as of activity of the organ, the mucus discharged from its surface is impregnated with the azotized digestive principle. It is the acid fluid poured out by the glandular follicles that is secreted only, when the stomach is stimulated by the contact of foreign matter against its surface. But without this acid fluid the animal principle which the mucus contains is of no avail towards the process of digestion.

Even under the severest forms of disease pepsine is still to be met with in the mucous membrane of the stomach. Taking seven stomachs from the post-mortem room of Guy's Hospital, which were derived from patients who had died respectively from typhoid fever, phthisis, apoplexy, amyloid degeneration, &c., lithotomy, ovarian disease, and heart disease with typhoid symptoms, I found that each yielded me an infusion, which, acidulated with the usual amount of hydrochloric acid, possessed energetic digestive properties, as shown by the attack made upon the tissues of the frog.

I consider that what I have just been stating has a bearing of importance in reference to the administration of pepsine as a medicinal agent. If pepsine thus exist in the mucus of the stomach instead of being produced by a special secretive action taking place only under the influence of food, doubts may arise as to the real advantage to be derived from its artificial preparation and exhibition with the view of aiding the digestive process. Leaving this, however, as an open question—for it is quite possible that under certain circumstances the mucous surface of the stomach, like other mucous surfaces, may secrete a low form of material which is but ill adapted for the purposes required of it,—I say, leaving this part of the question an open point, I will proceed to submit for your inspection experimental results which will give you an idea of the amount of real assistance that can be expected to be artificially given through digestion towards assimilation by specimens such as these, with but a single exception—specimens that I purchased from our leading establishments in London as pepsine, or the digestive principle.—*Lancet, Dec.* 6, 1862, *p.* 613.

34.—ARSENIC IN ALBUMINURIA.

By J. Wearne, Esq., Helston, Cornwall.

About a year ago I had under my care a miner, aged about forty, of robust form, but of dissipated habits, and who had latterly led a hard life. His complexion was pale and doughy; voice husky; spirits depressed; urine small in quantity in the earlier part of the case, but becoming a little more plentiful under the use of diuretics. At length anasarca began to show itself in the feet, and subsequently there was distinct ascites. These symptoms were frequently relieved by the use of the compound jalap powder, but only for a short time. Our miners seldom suffer from albuminuria; but my suspicions becoming aroused, I examined the urine, and found it charged with a great amount of albumen. I have before now been shown albumen that I could not see, simply because, like the Spanish fleet, "'twas not in sight." But in the instance of my miner there could be no mistake. I immediately commenced the use of the sesquichloride of iron, and gave a good diet of beef and beer.

After a fortnight's trial finding no benefit from this line of treatment, I had intended a trial of the bichloride of mercury, but subsequently gave him Fowler's solution of arsenic, with the most marked and decided advantage. He began to improve in less than a week, his spirits began to return, his appetite to improve, and the albumen to disappear from the urine. After having satisfied myself and others of the change, I very soon discontinued the arsenic, with the effect of restoring the albumen to the urine. Being now convinced that the solution was indispensable, I again gave it, and continued it for about three weeks. The albumen again disappeared, the whole of the dropsical symptoms were entirely relieved, and the man went back to his labour, where he continues to work hard, and to lead his own kind of life, half miner, half navvy.

I have not had an opportunity of repeating this treatment; but should not hesitate to try it again in any case where the disease had not made too great a progress in undermining the powers of the heart and brain.—*Lancet, Feb.* 28, 1863, *p.* 257.

35.—ON THE USE OF TRITICUM REPENS IN IRRITATION OF THE BLADDER.

By Henry Thompson, Esq., London.

An infusion of the underground stem of the Triticum Repens (dog's grass, or couch grass) is an agent from which I have derived much advantage. For 12 months I have used it largely, both in private and in hospital practice, in numerous cases of constant and severe irritation of the bladder from any cause: and in a certain proportion it has been of great value. It has long been prized in these circumstances in some country districts; and it is officinal as a diuretic, and as a "diet drink" among the French. My first knowledge of it was derived from a country patient suffering from severe stricture, and the frequent and painful micturition of which he was the subject was obviously more effectually subdued by this remedy, which he had long taken for the purpose, than by any medicinal means I could devise. Accordingly I have tested its powers as above stated; and my belief is, that it is of more value in the cases described, and in those of renal calculus especially, than in prostatic irritation. Nevertheless, in these cases also, where the indications are to lessen the frequency and the pain of micturition, it is unquestionably useful; often affording relief when buchu, pareira, uva ursi, and other infusions, have failed. The mode of administering it, which I have adopted, is the following. One ounce of the dried underground stem, popularly called root, is infused in a pint of boiling water for an hour, the liquid is strained off and taken when cool; from half a pint to a pint to be taken in 24 hours. It may be used as a vehicle for many other agents, but I have almost invariably given it alone, with the view of ascertaining its effects. It is somewhat diuretic, and perhaps slightly aperient.—*Dublin Quarterly Journal of Medical Science, Feb.,* 1863, *p.* 115.

SURGERY.

DISEASES OF BONES AND JOINTS, AMPUTATIONS, ETC.

36.—ON THE IMPORTANCE OF MECHANICAL REST IN THE TREATMENT OF DISEASES OF THE KNEE-JOINT.

By John Hilton, Esq., F.R.S., Surgeon to Guy's Hospital.

In all cases of accidental injury or of inflammation of the knee-joint, as well as in every disease of this articulation, the knee should be kept uninterruptedly at rest and straight, or nearly so, until its reparation is complete; and I believe that as soon as sufficient importance is given to such views we shall cease to witness those numerous and extraordinary deformities about the knee-joint, resulting entirely from the want of mechanical resistance to that muscular force, which induces excessive bending of the tibia backwards, or a dislocation of the tibia and fibula outwards and backwards, of which you find so many specimens in the metropolitan and other pathological museums. A fixed rigid splint will certainly prevent such deformities—I feel positive about it—provided the splint be sufficiently resisting. A gutta percha splint will not accomplish it, not being sufficiently strong. A firm, padded splint of leather, wood, or iron ought to be placed along the posterior and lateral part of the joint; and I may add that in all such cases it is advisable that the limb be swung, as in cases of fractures of the leg. During many years past I have swung nearly all my cases of acute disease of the knee-joint, and I have found it contribute immensely to the comfort of the patient.

Here are two or three cases in illustration of the deformity which ensues from the non-employment of mechanical resistance to disturbing muscular force, and of the method of proceeding to be adopted in order to remedy the deformity which may have occurred.

Case of diseased knee-joint; the knee flexed, and the tibia partially dislocated backwards; the biceps tendon divided, and the *limb straightened; cured by "mechanical rest."*—A young lady, J. M., when she was two years old had a fall upon her right knee, and she suffered afterwards from symptoms of acute inflammation of the joint, followed by contraction of the hamstring muscles, and extreme flexure of the knee. Two abscesses, one in the popliteal region, and one above the knee, were associated with this diseased joint. She has not been able to walk without crutches since the accident, has employed very inconvenient instruments for the purpose of putting the leg straight, and thinks she received more harm than good from their application. On March 29th, 1860, she came under my care in Guy's Hospital; she was then eighteen years old. When standing upon the left leg, the heel of the right foot was between four and five inches above the ground. The patella was fixed firmly by bone to the femur. Chloroform was given by the house-surgeon, and forcible extension was employed, but we could not straighten the limb; the tendon of the biceps muscle was therefore divided about one inch before reaching the head of the fibula; forcible extension of the knee was continued, and the limb straightened, after which a straight wooden splint was applied to the limb upon its posterior aspect. After the operation, ice in a bag was constantly applied to the knee, in order to prevent the occurrence of any inflammation, and five grains of Dover's powder were ordered to be taken at night. On the 13th of April, within a fortnight of the forcible extension, her leg was straight, and very comfortable. The splint was constantly applied until May 21st. The patient is now about twenty years old, and can take a considerable amount of exercise without difficulty.

In this case I did not find it necessary to divide the tendons of the semi-tendinosus or the semi-membranosus muscles; these after the division of the biceps, yielded to the influence of extension.

Before operating upon the tendons of the inner hamstring muscles, it is necessary to bear in mind, and to ascertain, the exact relation of the posterior tibial nerve to them. Previous to the administration of the chloroform, there is usually on difficulty in distinguishing the real characters of the tense cords which you may feel passing along the tightened part of the popliteal region, as we see it when the knee is much bent from old disease of the joint. The posterior tibial nerve may then be recognised by the pain which is induced by pressure being made upon it. The course of the nerve being ascertained, the other cords must be the tendons; and further than that, as far as I have observed, upon closely examining the parts, you may trace the individual tendons up towards their respective muscles, and so identify them as tendons.

Old diseased knee-joint; knee bent; tibia displaced backwards; flexor tendons divided; treated by "mechanical rest."—Martha M., aged fourteen, was admitted into Guy's Hospital, under my care, in January, 1860, suffering from the effects of old-diseased right knee-joint. The tibia was drawn backwards and upwards, so that she could only just touch the ground with the great toe by inclining the pelvis to that side. At five years old, when quite healthy, she had her knee struck by a little hammer; the knee became chronically inflamed. She had no splints; and although she was under constant professional treatment, she continued to get worse during two years, when she was taken to a London hospital with advanced disease in the knee-joint. She remained there seventeen weeks in bed, and during the latter part of the time she had scarlatina severely. A gutta percha support was applied to the knee, yet the knee became more and more flexed, so as to form nearly an acute angle with the posterior part of the thigh. The bending of the joint occurred chiefly during the time she had scarlatina. The mother says there never was any discharge from the joint; two cicatrices exist, which she says "were made by the surgeon to draw off the corruption, but he found that there wasn't none." The surgeon proposed to amputate the leg as soon as the health of the patient became good enough, but the parents would not entertain that question; so she was allowed to get about at home on crutches up to the time of her admission into Guy's, with the right leg useless, because the knee was flexed and the tibia displaced backwards, the condyles of the femur projecting forwards. The patella was firmly anchylosed in a good position upon the condyles of the femur; the tibia was movable upon the femur to a slight extent, with membranous anchylosis between the tibia and femur. Chloroform having been administered, the leg was extended, and the tendons of the biceps, semi-tendinosus, semi-membranosus, and gracilis were divided. The leg (tibia) was forcibly drawn first downwards and then forwards, so as to overcome or correct the deformity resulting from the displacement of the tibia backwards. In this we were only partially successful, although some tearing of the tissues was heard during our efforts at extension. The limb was then bandaged upon a straight splint; the tibia, however, could not be brought to its proper position as regards the femur. In ten days another splint was applied, with a screw attached to it, so that the knee could be gradually extended and the tibia advanced until the limb was nearly in its present condition. She left the hospital eighteen weeks after admission, with the screw splint upon the leg. As soon as the limb could be maintained in that improved position by the patient, a common leathern splint was applied to it, for the purpose of supporting and defending the joint. This leathern splint was worn day and night for fourteen months, during which time she has walked more or less on the lame leg, gradually increasing the distance and going to school daily, with her health improving.

These are cases which, I think, maintain the conclusion that "physiological rest" (by division of tendons) is an important agent to be employed in correcting some of the deformities of diseased knee-joints, which might be as surely prevented by the application of splints in the early part of their treatment; the object being at that period to oppose successfully those muscles which we know by experience may displace both the tibia and fibula backwards and outwards, and so produce the numerous cases of deformity which require so much operative and mechanical surgery for their correction.—*Lancet, Nov.* 15, 1862, *p.* 525.

37.—ON EXCISION OF THE KNEE-JOINT.

By Richard G. H. Butcher, Esq., Surgeon to Mercer's Hospital, &c., Dublin.

[Mr. Butcher is the first authority in the world on the subject of excision of the knee joint. The following rules are adhered to by him in performing this operation.]

1. *The judicious selection of the case.*—The bones not being diseased far beyond their articular surfaces, while if upon section found to be a little more than had been expected, the part should be gouged out, or an additional thin slice removed; but if to a greater extent amputation should be at once resorted to, and as recorded in my first memoir with a hope of excellent success ("First Memoir on Excision of the Knee Joint, page 64"). Again, the report goes on to show that amputation may be performed some days after excision should any unfortunate circumstance in the management of the case have arisen to demand it. In this same paper seven instances are recorded of amputation of the thigh, and all made rapid recovery save one (page 65).

2. *The H incision should be preferred.*—The perpendicular strokes placed well back so as to allow all fluids and discharges to drain off; far more effective and safer than any opening made in the popliteal space. No portion of the flaps to be curtailed, though they may be thinned of any thickened fibrinous matter or diseased synovial membrane; the latter, particularly, should be clipped away with a strong scissors; all ligamentous fibres, both around and within the joint, should be cut through, and the extremities of the bones fairly freed and exposed.

3. *The patella should be taken away, in all cases, whether diseased or not*, and then the section of the bones well thrust out in front,

should be made with "Butcher's saw," from behind forward, due attention being paid to the axis of the thigh bone at the time of its division.

4. *All bleeding vessels should be tied, or any that have sprung and retracted should be drawn out and secured*, so as to guard against intermediary hemorrhage.

5. *While the patient is yet on the operating table, the limb should be placed in the horizontal position, either by gentle and steady traction, combined with pressure of the cut surfaces of the bones backwards, or, if necessary, the division of the hamstring tendons.* Their support behind, in every case, I look upon as of great value: therefore, their section must be looked upon as a last expedient towards straightening the limb.

6. *During the adjustment of the bones, great caution should be exercised that their surfaces be throughout their extent in contact, and that no soft parts intervene.* The flaps should then be laid down and connected by suture closely throughout their transverse division, while the lateral incisions should be brought together only at their extremities by one or two points, and the central portion of each, that corresponding to the division of the bones, should not be brought in contact, but dressed lightly with lint soaked in oil, thus securing a ready outlet for the escape of fluids. The extremity should next be cautiously laid upon "Butcher's box splint," padded to the natural configuration of the limb, its sides elevated, foot-board applied, suitable pads introduced, and then the anterior splint laid on, taking the place of the assistant's hand, which from the first restrained the femur from projecting forward; then the straps buckled, the waist-band applied, and the patient may with safety be removed to his bed. The bed should be prepared in this way, and consist of a couple of hair matresses laid one upon the other, evenly supported, and intervening between the upper one and the sheet, a folded blanket, feather pillows for supporting the head and shoulders; the bed should be likewise moderately warmed so as to prevent the patient being chilled when put into it.

7. *The limb should not be disturbed for several days*, the length of time depending a good deal on the season of the year when the operation is performed, whether it be in the heat of summer or the cold of winter. After five or six days it may be necessary to let down the sides of the box-splint, to sap up discharge, change lateral pads, and soiled dressings, &c. By the apparatus named the facilities for cleansing the limb are so efficient that it may not be requisite to lift the member from its support for even so long a period as five weeks, as evidenced in my own practice. Should, however, it be considered expedient to change all the dressings, the anterior splint should be steadily held back by all assistants, and the limb pressed up to it, thus

guarding against any starting of the femur forwards or displacement laterally when lifted from its bed. When the box is prepared freshly arranged, the limb, controlled after the manner mentioned, should be laid down, the side-splints elevated, foot-board secured, and the straps over the anterior splint first tightened, so as to maintain it in that position, from which it is never suffered to change. I would impress the advice still further, if the straps be unloosed for any purpose, *the hand of an assistant should steadily keep the anterior splint in its position* and well pressed back, until the artificial support is again brought to bear upon it and fastened.

8. *In cases where large abscesses form in the vicinity of the excised joint, up along the thigh, Chassaignac's drainage tubes may be used with the best hopes of success.* (See Butcher's reports on "Operative Surgery," *Dublin Quarterly Journal*, February, 1859).

9. *The free administration of stimulants and sedatives, imperatively demanded in all cases of excision*, regulated to a certain extent by age, sex, temperament and habit.—*Dublin Medical Press, Feb.* 11, 1863, *p.* 133.

38.—ON THE NATURAL HISTORY AND TREATMENT OF HIP-JOINT DISEASES.

By Richard Barwell, Esq., Assistant Surgeon to the Charing-Cross Hospital.

You will see in books, only two or three years old, various assertions of hip-joint disease beginning in all sorts of structures —in the cartilage, in the round ligament, in the acetabular fat. Now, take my word for it, and look elsewhere for the proof, that no surgical disease of a joint begins in other structures than in the synovial membrane (including sub-synovial tissue) and in the bone, and that the hip does not differ in this respect from the other articulations.

In the commencement of the first stage of *morbus coxæ* it is quite possible to distinguish the synovitic from the osteitic disease. The symptoms of the latter are as follows :—The appearance of suffering and the worn look in children come on before any limping can be detected, and before any complaint of local pain can be elicited. When any such complaint is first made, the knee is nearly always pointed out as its seat ; afterwards the hip becomes painful, in which place the pain increases throughout the day, being worse at night. There is no stiffness in the morning. The pain at the knee is more variable, but is always worse at night, and is early accompanied by starting of the limb. These symptoms occasionally disappear for a few days. There is no tenderness on pressing behind the trochanter, nor, unless inflamed from other causes, in the groin. Tenderness from pressing the joint surfaces together is not uncommon. During the progress of these symptoms a very gradual change takes place in the position of the limb and in the gait of the child : the foot is turned a little out, the toe is pointed and drags, and limping becomes perceptible. At this time, too, there is often difficulty and pain in abducting the leg. There is no swelling either behind the trochanter or deep in the groin. The inguinal glands are, however, frequently enlarged, and this is a point which should strongly influence the surgeon's opinion. There is no increased heat about the part.

By carefully contrasting one set of symptoms just described with the other, you will have little difficulty in distinguishing the two cases; and your diagnosis will be assisted by remembering that the osteitic disease attacks children almost exclusively, and them only in a considerable state of strumous cachexia; it hardly ever comes on in persons approaching puberty, and in those beyond that period hip-joint disease begins always in the synovial membrane. When the surgeon has convinced himself that there is disease of the hip-joint, his next consideration is the treatment that he shall apply: and in either case, whether the commencement of the disease be in the bone or in the synovial membrane, the great resource of his art at that period will be, rest. In a good many of the cases seen quite early simply keeping the patient in bed for about three weeks will alleviate all the symptoms—at least for a time—perhaps for a month or two, perhaps for a year. The same thing may be said *à fortiori* perhaps of the simple application of a long straight splint. Every surgeon with any experience in hip-disease knows that he may in the slighter cases, particularly in those that begin in the synovial membrane, thus conquer the symptoms for a time; but he also knows that a fuller and more careful treatment is frequently required even to effect such temporary restoration, and is nearly always necessary to produce more permanent benefit.

I have frequently been able to show you the difference in condition and appearance of the struma with thick connective tissues and the struma with fine tissues. I have proved to you by actual experiences that a good many of the sluggish inflammations to which the former constitution is liable are in their beginning to be greatly benefited by a purge, and we have seen that a purge containing some mercurial has a particularly good effect, clearing away the tenacious mucus from the surface of the bowel, thus allowing the food, and any necessary medicines, to act. We may after this find a few doses of quinine and grey powder desirable, and in some form or other iodine will be nearly always useful. Cod-liver oil, sarsaparilla, or other nutrient forms of tonic, as a rule do harm. On the other hand, in the fine-tissued and delicate-tinted struma, which develops a more rapid and differently constituted inflammation, a very different treatment is necessary; the bowels will probably require regulating, but this must be effected by mild vegetable aperients, such as a little rhubarb, taken with the food. Iodine is generally to be avoided. Cod-liver oil and other nutrient tonics are useful, as also iron and quinine.

Let us return to the local treatment of the disease, and take up the subject where we left it off—with rest. The term applied to a joint does not mean merely that it is not to be exercised, but that it is to be kept as nearly as possible immoveable. This with regard to the hip is a matter of very considerable difficulty; for instance, however well we fix the limb upon a "long splint" (a Desault's), the other side of the pelvis may be lifted or depressed, or otherwise disturbed, producing movement of the acetabulum on the head of the bone. I think there is little doubt but that rest very nearly absolute may be obtained by Bonnet's "*grand appareil*," an imitation of which in America by Dr. Bauer has obtained the name of wire breeches. But in thus producing such rest for the hip, we also confine the other limb and the whole lower half of the body, including the loins, and the irksomeness of the imprisonment is sometimes unbearable. Many surgeons have recommended a double inclined plane, such as an Amesbury bed. This I consider, for reasons which will be shown, very defective both in principle and in practice. Thick spica bandages, much stiffened with starch or dextrine, are frequently employed; but they are not satisfactory, since the more or less distended or flaccid state of the abdomen, particularly in children, prevents a sufficiently close adaptation. Moreover, the material is neither sufficiently firm to resist movement, nor durable enough to outlive the inflammatory period of the disease; hence the very great evil of frequent reapplication. Yet I have felt so strongly that this bandage contains the right principle of enveloping the diseased thigh and the pelvis in such wise as to prevent mutual movement, that I constructed a splint for hip-joint disease upon that principle, and yet so as to avoid the defects of the bandage. You have not unfrequently seen this splint in use, and I can strongly recommend it to you, as the more I use it the more satisfied I am with its performance. It consists of a thigh splint long enough to reach from the crest of the ilium to below the foot. At an obtuse angle to its upper end is fastened a pelvic portion, made of wire gauze, strengthened at its edges by stronger wire, or of perforated zinc; this passes round the pelvis just below or on a level with the crest of the ilium from one spinous process to the other; the two ends are connected by an india-rubber cord, passing across the belly,* securing the close fit of the pelvic portion. The whole splint is to be bandaged on the limb from the foot upwards. I know of nothing that will keep the thigh fixed upon the pelvis in a straight position with such security, and with so little irksomeness to the patient, as this splint.

Although, as I have already said, a good number of patients will get well temporarily, and some permanently, under this management, yet there are some who will not do so; but the swelling and the pain still increasing, demand some further treatment. There are not many local applications that can be made to the hip, and of the value even of these few there are great differences of opinion, arising, I believe, from a want of clear definition of the various phases of the malady to which one or the other is suitable; for an application that would be very valuable in one stage of the disease would be worse than useless in another. Moreover, there are certain points relating to the mode in which the disease commences, and which must have great weight in guiding us to a choice of remedies.

When synovitis is the starting point of the malady, counter-irritants, if used at all, should be of the most superficial and slight character; for delicate children, even a blister is too severe. Such applications as the French "papier vesicatoire," a little of the liquor vesicatorius, even tincture of iodine, are useful, or, what is better than all these, the nitric acid and silver lotion,† or an ammonia soap.‡ After all, however, tonic regimen and change of air, combined, of course, with properly guarded rest and skilful attention, will do more than local applications. Yet I must warn you, that although all the violent counter-irritants are to be avoided, the opposite extreme of what are called soothing remedies are quite as hurtful. Do not keep poultices or any sort of moist warmth applied over any chronically inflamed joint; for nothing is more conducive to a loose, flabby, congested state of parts, which is often an immediate precursor of, and always tends to, chronic inflammation. Dry heat, by means of hot salt-bags, or heated plaster of Paris, first cast to the shape of the haunch, is a much better remedy; but the heat must be sufficient rather to stimulate than to relax. When the disease commences in the bone, the child very rarely comes under surgical care at an early period—that is, when limping can only occasionally be detected, and then by a practised eye, and when complaints of any local pain are neither frequent nor much insisted on. There is but little doubt that if we could get our patients as soon as the disease appears, we might cure a very large proportion of them by rest and bracing treatment; but when the malady has reached far enough to produce decided local pain (probably first in the knee), evident limping, and change in the position of the limb, we shall not master the morbid action so easily. When in such cases it be found that mere rest is not productive of benefit, but that the child wastes and the pains become more severe, further measures are imperative. The symptoms are, as I have elsewhere taught, evidences that the bone stricture immediately underlying the joint surface is diseased.

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* The cords, &c., which pass round it do not interest us at present, but will be adverted to in a future lecture.

† Nitrate of silver, half a drachm to two scruples; strong nitrous acid, two to three minims; distilled water, one ounce.

‡ Strong liquor ammoniæ, one drachm; tincture of cantharides, fifteen minims; olive oil, one ounce.

But it is now my desire to mention a remedy which, in my experience, never fails to banish entirely these symptoms—always for a time, and occasionally altogether: this is the actual cautery. The iron which I use for this purpose is a triangular prism, mounted on a crooked stem. It is heated to a *white* heat in a charcoal fire. The patient is to be put fully under the influence of chloroform, and a line is to be drawn on the skin, with a gentle pressure and a rather slow passage of the iron, from the trochanter backward. Formerly I used to make two or three lines, but I am now convinced that all the advantages are to be gained, and some inconveniences avoided, by the less extensive application. It is right that I should tell you what the iron will do, and what it will not do. In the first place, it will not of itself cure the osseous inflammation, but it may place it in a good position for getting well. We shall see by-and-by that the symptoms are referable to muscular spasm producing articular pressure, and that the inflammatory disease is kept up by that condition. Now, the hot iron instantly dispels this spasm and its consequences, at least for an interval, during which the general health improves rapidly, and the local malady has a fair chance of getting well. So instantaneously is the subdual of all painful symptoms, that not uncommonly a child who would previously be almost day and night in anxious watchfulness, dreading beyond everything the slightest movement, can hardly, after the application, be kept from jumping about the bed in the highest spirits.

Do not, however, be deceived by such rapid and seemingly total success; there are few cases in which the pain does not return. The only permanent remedy which I find for it is extension, but the cautery may be very useful as an aid to that mode of treatment.—*Lancet, Nov.* 22, 1862, *p.* 557.

39.—CASE OF AMPUTATION AT THE HIP-JOINT.

By James Spence, Esq., Surgeon to the Royal Infirmary and Lecturer on Clinical Surgery, Edinburgh.

[The patient was only fifteen years of age, and was admitted into the Royal Infirmary on account of a large tumour occupying the middle and lower part of the right thigh. It first appeared soon after an attack of fever rather less than a year previously; it was painful and covered with enlarged ramifying veins. When admitted the whole circumference of the middle and lower part of the right thigh was occupied by the tumour. Mr. Spence performed amputation at the hip-joint in the following manner:]

The patient having been put under chloroform, Lister's tourniquet was applied to the abdominal aorta. The breech was brought well over the edge of the table, and the pelvis held steady by an assistant. The tourniquet was entrusted to Dr. Gillespie, while Dr. Watson compressed the femoral artery at the brim of the pelvis. Mr. Spence having abducted, and slightly flexed the limb on the pelvis, gave it in charge to Dr. Struthers. Then grasping the soft parts with his left hand, Mr. Spence entered the point of the knife in front of the tuberosity of the ischium, and passing it obliquely upwards in front of the articulation, brought out the blade almost midway between the anterior superior spine of the ilium and trochanter major, so as to form a large anterior flap, disarticulated the limb, and terminated by cutting a posterior flap nearly equal in size to the anterior. A large sponge was immediately applied to the posterior flap, until the large vessels in the anterior flap were secured. No artery jetted so long as the tourniquet was retained. After the arteries had been tied, and the tourniquet removed, the femoral vein continuing to bleed, a ligature was placed upon it. The flaps were then stitched together, and a pad placed on the anterior and posterior surfaces to keep them in apposition. The patient having been removed to bed, a beef-tea and brandy enema was immediately administered, and after she had rallied an opiate was given.

No occurrence worthy of notice interrupted the convalescence, and she returned to the country in a state of health bordering on that of former years.

In regard to the operation itself, in this particular case, I would only notice two points, and that very briefly. First, The mode adopted to restrain hemorrhage during the operation. This was effected by compressing the abdominal aorta by the tourniquet, or compressor, proposed by Professor Lister, of

Glasgow, and which he kindly lent me for the occasion. I had previously ascertained, that, from the thin condition of the patient, I could readily arrest the circulation by manual pressure on the lower part of the aorta, but an equal, unyielding mechanical compression is safer, and hence I preferred it; but, as it was a trial, I guarded against all risk from the anterior vessels by entrusting the care of the femoral artery to my colleague, Dr. Watson. The result, however, showed the efficiency of the instrument, as it completely commanded the usually incontrollable flow of blood from the branches of the internal iliac distributed to the posterior flap; and avoidance of loss of blood in an exhausted patient must have a most important bearing on the success of the case. How far such compression could be trusted to entirely in a strong man, or in the case of a very fat person, with a large abdomen, or whether it might, if incautiously applied, lead to injury to the viscera in certain conditions, further experience can only reveal; but certainly in this, and two cases in which it was used by Professor Syme, it proved perfectly effectual.

The other point of the operation I would refer to, was the necessity which arose for applying a ligature to the femoral vein. The hemorrhage from this vein continued after all pressure had been removed from the groin and abdomen, and as temporary pressure of its orifice and other means failed to arrest it, and as it returned whenever the pressure was removed, I found it absolutely necessary to tie it. I have frequently seen troublesome bleeding from the femoral vein in amputation of the thigh, but in general it stops when compression is made by applying the flap against its surface, when all pressure above is removed, and the stump elevated, but here these means proved ineffectual, or perhaps, I should rather say, the weak condition of the patient rendered it necessary at once to check all loss of blood. I confess that my prejudices against tying veins are very strong, and this step in the operation made me uneasy as to its effects; but as the history of the progress of the case shews, my fears were groundless, not a single bad symptom having followed ligature of the vein.

In conclusion, the uninterrupted progress of the patient's recovery was most satisfactory. It is not often that even a simple amputation of the thigh goes on so well; and whatever may be the estimate of the operation in general, or whatever may be the chances of the disease returning in some other organ hereafter, it has for the present at least, snatched the patient from the brink of the grave and restored her to her family, and to the enjoyment of perfect health.—*Edinburgh Med. Journal, Jan.* 7, 1863, *p.* 585.

40.—ON THE SUPERIORITY OF CHOPART'S OPERATION AND EXCISION OF THE ANKLE IN ALL CASES ADMITTING OF THEIR PERFORMANCE.

By Henry Hancock, Esq., Surgeon to Charing Cross Hospital.

Connected with this subject, there is no one in the United Kingdom to whom we are more indebted than to Mr. Syme, who in the year 1843 first introduced to the notice of the profession his celebrated operation of exarticulation of the foot at the ankle-joint. Since that period down to the end of the Crimean War, when Pirogoff promulgated his modification, Mr. Syme's operation remained in the ascendant, as Chopart's valuable method was not supported; and the choice consequently remained between amputation of the leg and Syme's operation, which he describes as follows :—

> "The incisions across the instep and foot should be curved, with the convexity forwards, and exactly opposite each other. A line drawn round the foot midway between the head of the fifth metatarsal bone and the malleolus externus will show their extent anteriorly, and they should meet a little way further back, opposite the malleolar projections of the tibia and fibula. Care should be taken to avoid cutting the posterior tibial artery before it divides into the plantar branches. The flaps thus formed are next separated from their subjacent connections, which is easily effected except at the heel, where the firmness of texture occasions difficulty. The disarticulation being then readily completed, if the ankle-joint be sound the malleolar processes should be removed by cutting pliers; but if the articulating surfaces of the tibia and fibula be diseased, a thin slice of these bones should be sawn off. ...... The flaps are then brought together by means of sutures, preserving the pad of soft parts, of the heel to form at the same time a covering for the end of the bones and a cushion whereon the patient could walk."

Great as was the acknowledged improvement of this operation over the old plan of amputation, it would seem to be open to objection. The irregularities of the protuberance of the os calcis, the intimacy with which the soft parts adhere thereto, and their comparative thinness at this point, render their detachment a matter of considerable difficulty, and they have not unfrequently been wounded in the attempt. Sloughing of the flap has followed in some instances, and among others in some of the earlier operations by Mr. Syme himself ; but in the Monthly Journal of Medical Science, 1862, he states that he had performed the operation fifty times without this accident occurring.

To obviate these objections, M. Pirogoff, in the year 1852, introduced his modification, now known as "Pirogoff's operation." This differs from that of Syme, inasmuch as the posterior flap is not formed solely by soft parts, but consists of the posterior tuberosity of the os calcis with the insertion of the tendo-Achillis. This flap is turned forwards, and the sawn surface of the os calcis brought in apposition with the cut surface of the tibia. The advantages claimed for this method are—that the tendo-Achillis is not divided; that the soft parts not being detached from the tuberosity of the os calcis, the difficulties of this part of Syme's operation—the danger of wounding the flap and its subsequent sloughing—are thus avoided; that, from the posterior flap being as it were solid and not cupped, accumulations of pus are not likely to form in that situation; and lastly, that the leg is an inch and a half longer, from the posterior tuberosity of the os calcis left in the flap becoming united to the end of the tibia and fibula, thus lengthening the limb to that extent, and serving the patient as the point of support.

This operation, however, is liable to be followed by suppuration in the sheaths and sloughing of the divided tendons; and although this to a certain extent may be obviated by well-directed pressure, it is nevertheless a point of the greatest importance in governing our selection between the two operations. In February, 1858, Mr. Croft, Assistant Surgeon to the Dreadnought, published in the Lancet the results of six cases of Pirogoff's operation—viz., four recoveries, two deaths; and it is well worthy of attention that whilst one patient died of granular disease of the kidney, the other died of secondary deposits of pus in various joints; and in all the four successful examples there was suppuration along the tendons of the leg.

There is still another and a very serious objection, however, which applies equally to both operations, and which should prevent our selecting them except in appropriate cases, viz., the necessary sacrifice of the foot and ankle-joint. But so great has been the prestige of these methods, so indiscriminately have they been employed, that we cannot read the hospital and other reports without feeling that the legitimate resources of surgery have not been made available to this region of the body, and I am sure that this feeling will not be confined to myself when it is learned that of forty-four cases submitted to Syme's and Pirogoff's operations which I have collected from various sources, in fifteen, or one-third, the disease is stated to have been limited to the ankle-joint.

This has arisen partly from prejudice against Chopart's operation, and partly from the mistaken notion, that excision of the ankle-joint is so difficult of performance and so hazardous that its adoption is scarcely safe or justifiable. The prejudices against Chopart's method, depending partly upon theoretical objection and partly upon error in the mode of performance, have prevailed, and a most valuable operation has consequently fallen into neglect. So, in like manner, resection of the knee, hip, elbow, and shoulder joints has frequently been performed; but we look in vain for resection of the ankle joint, so completely have the minds of surgeons been engrossed by these imaginary fears, and so entirely have they ignored the value of the foot, or a portion of the foot, as a part of the animal economy.

Chopart's operation, I need scarcely remark, is that whereby the anterior portion of the foot is removed at the joint extending between the calcis and cuboid and the astragalus and navicular bones, preserving the calcis and astragalus in their normal positions, and the ankle-joint intact. The objection, however, made against this operation is, that the extensor muscles of the ankle, having lost their opposing forces, and acting through the tendo-Achillis, draw up the heel, and direct the cicatrix towards the ground, whereby the patient, obliged to bear his weight upon the tender cicatrix, is prevented walking by the agony induced, and suffers so much that he willingly undergoes secondary amputation. Mr. Syme, who has performed secondary amputation at the ankle-joint in three cases, remarks; "You will observe that, as in all other cases of the same kind, anchylosis has taken place between the astragalus and calcis, whilst the latter has been previously drawn up by the action of the gastrocnemius, so as to prevent the patient from resting on the proper part of the stump.

In other cases, again, ulceration and exfoliation of the bone have occurred from tension of the flaps, necessitating the same untoward results.

Instances are also recorded in which, after this contraction had taken place, the tendo-Achillis was divided, in the hope of remedying the mischief, but with so little success that Syme's operation was subsequently performed. Mr. Fergusson relates a case of this character.

Nevertheless, I still advocate Chopart's operation wherever the disease is located anteriorly to the os calcis and astragalus, and where an adequate flap can be obtained. I have now performed this operation four times, with the best results, and I attribute this success to the following mode of proceeding :—

Making the upper flap at least an inch long, and carrying the under or plantar flap well on to the under surface of the toes, whereby, when there is much thickening, very nearly an additional inch is gained in that direction, and the junction of the flaps is brought to the centre of the stump instead of the upper margin, to which the principal stress is referred. The flaps, moreover, being full and free, danger of sloughing is avoided; whilst, if required, they provide a sufficient covering for the whole or a portion of the navicular bone, which, when possible, should always be preserved, as by this means we not only obviate the necessity of opening into the large synovial cavity of the astragalo-navicular joint, but we at the same time preserve the attachment of the calcaneo-scaphoid ligament, and consequently the natural and firm support to the head of the astragalus, whilst we also make the stump more full and even. Again, dividing the tendo-Achillis at the time of operation, and not waiting to do so until contraction has already taken place, when such division is useless, as the parts have now become adherent and fixed, and it is too late to remedy the mischief.

The success, moreover, is greatly influenced by the situation at which the tendon is divided. When this is done near to the os calcis, where the fascia extending from the tendo-Achillis on either side to the malleoli is dense and strong, and when the inability of the patient to rest his heel or foot on the ground during the antecedent disease has already induced a considerable amount of contraction, the consequent separation of the divided tendon is so slight that it quickly reunites, and the result is not to be relied upon. I always, therefore, select a point as near to the body of the muscle as practicable, where the fascia is less dense, and where the tendon is more under the influence of the muscular fibres.

Chopart's operation, performed in accordance with these suggestions, will prove as successful as any in surgery, and in support of this assertion I would refer to the following case:—

C. R., aged twenty-two, organ-builder, admitted Jan. 1855, into Charing-cross Hospital, under my care, with tarsal disease of the right foot. About the middle of March, 1852, he first noticed pain in the foot. He had struck the part a short time previously, but did not pay any attention to it, thinking it was only bruised. In a short time, however, the pain and inflammation became very severe, and Mr. Harvey, under whose care he was, ordered the part to be kept at rest, and leeches to be applied; and this treatment enabled the patient to return to work. Unfortunately he had to run up and down ladders very frequently, and in so doing often struck the foot. Consequently, in the May following, he was again compelled to relinquish work, an abscess having formed on the dorsum of the foot, over the navicular bone. This was opened by Mr. Harvey, and a quantity of offensive matter discharged. The discharge continued till the following December, when he came into the hospital, and I enlarged the opening, at the same time removing a portion of the scaphoid bone. The wound healed, and he was sent to the seaside. In April, 1853, the disease reappeared; and in

January, 1854, I again removed a portion of the scaphoid bone, and he returned to the seaside apparently cured. He remained tolerably well until the autumn, when abscess again formed. As the pain increased, and his health became impaired, he was readmitted in January, 1855, soon after which I performed Chopart's operation as follows :—

I commenced an incision on the inner side of the foot over the scaphoid bone, carrying it forwards about half an inch, and then continuing it in a semilunar direction across the dorsum of the foot, terminating opposite the prominent end of the metatarsal bone of the little toe. This incision penetrated to the bones throughout. The long amputating knife was then passed on its flat across the sole of the foot from the outer to the inner extremity of the former incision, as close to the bones as possible, and the under flap then made by carrying the knife forwards a little beyond the roots of the toes. The flaps thus formed were dissected a little backwards, the joints between the astragalus and scaphoid and between the calcis and cuboid bones exposed, and the ligaments having been cut through, the parts were removed, and the arteries secured. I then made a subcutaneous section of the tendo-Achillis, about two inches and a half above the os calcis, after which the flaps were brought together by sutures, a bandage applied, and the patient returned to his bed. He made an excellent recovery, and left the hospital cured in two months.

I have frequently seen this patient since. He is now quite well, and able to walk and run up ladders as well (he says) as ever he did. The plantar surface of his stump is perfectly horizontal, and the heel rests properly on the ground in standing or walking. Having had an artificial front of cork made to the stump, he wears a common boot, and sometimes walks several miles in the course of the day.

[The ankle-joint was first excised for disease in 1792 by Moreau, and since then has been done repeatedly on the Continent, but in England it has not met with the support it deserves, and though Mr. Teale and Mr. Wakley have removed the os calcis and astragalus, there is no record of true excision of the ankle-joint prior to 1851, when Mr. Hancock performed the operation for the first time.]

I have now excised the ankle-joint in five cases; four times successfully, once unsuccessfully, the patient dying seven months after the operation from phthisis, induced by dissipation. My colleague, Mr. Barwell, has also performed the operation. When we consider what has been done of late years in the surgery of other joints, when we also consider how much we may add to the comfort of, and what substantial benefit we may confer upon, the patient by this operation, we may fairly inquire why a solitary exception is made in the case of the ankle-joint, and why the foot is so frequently and so unnecessarily sacraficed. Are the difficulties of this operation greater than those of Syme and Pirogoff, and is it more hazardous to the patient?

A very exaggerated notion would seem to obtain upon this point. Even Mr. Fergusson, at page 448 of the fourth edition of his "Practical Surgery," says: "Under any circumstances, I should consider such operations extremely difficult, and in most instances more dangerous to the patients than amputation at the ankle or leg." When such an opinion is expressed by so eminent a surgeon, it is not surprising that the operation should be regarded with disfavour; but I confess I do not understand in what this extreme difficulty or this great danger consists. When performed in the following manner, it is neither more difficult nor more dangerous than the generality of operations in surgery.

Commence the incision about two inches above and behind the external malleolus, and carry it across the instep to about two inches above and behind the internal malleolus. This incision is merely to divide the skin, and should not on any account penetrate beyond the fascia. Reflect this flap; next dislodge the peronei tendons from the groove at the back of the external malleolus, and cut through the external lateral ligaments of the joint, carrying the knife close to the edge of the bone. Having done this, cut through the fibula, with the bone nippers, about an inch above the malleolus; remove this piece of bone by dividing the inferior tibio-fibula ligament, and then turn the leg with the foot on to its outer side. Now carefully dissect the tendons of the tibialis posticus and flexor communis digitorum from behind the internal malleolus, and, keeping the knife close around the edge of this process, detach the internal lateral ligament; then, grasping the heel with one hand, and the front of the foot with the other, forcibly turn the sole of the foot outwards, by which the lower end of the tibia is dislocated and protruded through the wound. This done, cut off the end of the tibia with the common amputating saw, and then, with a small thin metacarpal saw, introduced between the tendo-Achillis and the upper articulating surface of the astragalus remove the latter by cutting horizontally from behind forwards. Replace the parts *in situ*, close the wound carefully in front of the ankle, but leave the sides open to allow the discharge a free exit; apply water dressing, place the limb on a T splint, and the operation is completed.

There are no vessels divided in this operation, and, for obvious reasons, the greatest care should be taken not to wound either the anterior or the posterior tibial arteries. The anterior is avoided by confining the incision to the skin in front of the ankle, and we may easily avoid the posterior by keeping the knife close to the inner malleolus whilst separating the tendons and cutting through the ligament. The greatest difficulty which I have experienced has been in getting the external malleolus away; but this may be facilitated by twisting the detached portion by means of the strong bone forceps, so as to bring the ligament into view and admit of its division. I would also urge the superiority of the mode of making the flap here described over the vertical incisions behind the malleoli, as advised by some authors. Where much disease has existed in the neighbourhood of the joint, there is usually so much thickening of the soft parts that we get no more opening than what we obtain by our cuts. We are consequently cramped for room; we have not the opportunity of properly ascertaining the extent of disease, nor the condition of the parts we cut through; whilst the consequent amount of bruising is extremely prejudicial to the patient.

I have already alluded to the risk attending Pirogoff's operation of subsequent suppuration in the sheaths of the divided tendons and sloughing of the latter, as exemplified by the six cases published by Mr. Croft. Regarding this point, Pirogoff says: "I fear nothing so much as this—namely, when the belly of the muscle contracts and draws up the tendon, divided or half destroyed by suppuration, out of the sheath." Neither is Syme's operation free from this risk. In the two cases related by Butcher, one had a succession of secondary abscesses, the other had pyæmia, and was only saved by the skill and energy of that distinguished surgeon; whilst, at page 487 of the fourth edition of his "Practical Surgery," Mr. Fergusson remarks: "What with the violent inflammation extending up the leg, sloughing, secondary hemorrhage, death immediately dependent on the operation, tardy healing of the wound, and defective condition of the stump, from languor of circulation and tenderness, so that it could not be pressed upon or made useful, I have formed a most unfavourable impression against it."

Excision of the ankle-joint is for the most part free from these defects. The nerves and arteries being preserved intact, sloughing does not occur; whilst the non-division of the tendons, and more especially the non-interference with the extensive sheath of the tendons in front of the joint, exempt the patient from the danger of suppuration in the sheaths and sloughing of the tendons, as well as from that danger of which Pirogoff appears to have so lively a dread.

In neither of my own cases has there been any sign of sloughing of the tendons or suppuration in their course, or of anything approaching to pyæmia. In one or two there was slight inflammation of the lymphatics, which subsided in a few days; and the amount of discharge has certainly not been more than, if so much as, what commonly follows excision of the knee-joint.

Nor are those cases where, as Mr. Syme has observed, the disease is situated mainly in the articulation between the astragalus and os calcis unfavourable for this operation. In my fourth case this complication existed. I removed the whole of the astragalus, behind its neck, and the diseased portion of the os calcis, and the child made a perfect recovery. More recently, Dr. Canniff, of Toronto, performed a similar operation, removing the whole of the astragalus and the diseased surface of the os calcis with great success.

But if we meddle with the ankle-joint at all, never mind how limited the disease may be, we should always excise the entire joint, and not a portion merely. We can never judge from the disease on the surface to what extent the mischief in the cancellated structure has proceeded; and, however carefully we may endeavour to gouge the disease out, we rarely succeed so completely that some portion is not left behind, in addition to which we inflict an injurious amount of bruising.

In these partial resections we expose our patients to the dangers attending wounds of joints, whilst by complete excision the joint structure is got rid of, bone is brought in direct contact with bone, and the process of cure is rendered more simple and more certain. Moreover, in these partial proceedings we do not afford that free exit for discharge, we do not prevent those confinements of matter, which are so prejudicial and dangerous in all operations connected with joints.

Another benefit resulting from total excision of the joint is that it enables us to employ the saw, which should always be used, where practicable, in preference to the gouge. We thereby obtain even surfaces, we bruise the parts less, and we are also enabled to form an opinion as to the soundness of the bones themselves.

The recorded cases in which the constituents of the joint were removed at different times, and those also in which the bones or portions of the bones were gouged away, do not by any means afford satisfactory results.

(*a*) Mr. Statham excised the astragalus. A year afterwards he removed the two malleoli. The parts, however, never healed, and the man suffered so much that, at his own request, Mr. Bowman amputated the leg three years after the first operation.

(*b*) Mr. Price gouged away the astragalus and the horizontal articular surfaces of the tibia, but left the malleoli. Eight months afterwards he was obliged to operate a second time.

(*c*) In another case Mr. Price gouged away the astragalus and articular end of the tibia, the posterior part of the cuboid, and a portion of the fibula. Three months afterwards he amputated the leg in consequence of abscess.

(*d*) Mr. Hussey reports (August, 1858) that in the January preceding he removed the end of the fibula, but, in consequence of anchylosis, he could not bring the end of the tibia out to enable him to saw it off; he therefore used the gouge to remove the end of the tibia, and upper surface of the astragalus. Six months afterwards he amputated the leg.

(*e*) In June, 1852, Mr. Solly gouged away the external malleolus, a portion of the astragalus and articulating end of the tibia of left foot. On July 2nd he performed the same operation on the right foot of the same patient. On Oct. 2nd he again operated upon the left foot; and in April, 1853, he again operated upon the right. The patient ultimately recovered.

(*f*) On June 18th, 1852, Mr. Solly gouged way carious bone from the end of the tibia and astragalus. On July 10th, 1852, he amputated the leg.

Is the operation followed by an excessive rate of mortality?

Paul, in his "Consertive Chirurgie der Gluder," has gathered 719 cases of amputation of the leg followed by more than one-third, or 249 deaths.

Of 8 patients operated upon by Mr. Fergusson, according to Syme's method, 2, or one-fourth died.

Of 6 cases reported by Mr. Croft, as operated upon according to Pirogoff, 2, or one-third, died.

Of 25 cases of Syme's operation, collected from various sources, there were 19 cures, 3 deaths, 2 secondary removals of ends of tibia and fibula, and 1 sloughing of integuments.

Of 14 cases of Pirogoff's operation, 2 never healed, 2 died, and 10 recovered.

Of the 15 cases of disease of the ankle-joint to which I have already alluded as having been submitted to either Syme's or Pirogoff's operations, 9 recovered, 1 died, 1 underwent a secondary operation for the removal of the ends of the tibia and fibula, 1 had sloughing of the integuments, 2 did not heal, and the result of one is not known.

Of the 5 patients upon whom I have operated for excision of the ankle-joint, 1 died seven months after the operation from phthisis; whilst of 33 cases collated from various sources I find that 21, or two-thirds, recovered absolutely, 7 suffered secondary amputation, and 5 died; but of these 3 are stated to have died of phthisis, one seven months, one three, and the third eight days after the operation. If the account of the two latter be correct, it might perhaps have been more judicious if the operations had not been performed at all. As regards the 7 secondary amputations, I have already shown that in 4 the joint was not entirely excised, but that portions were allowed to remain.

Lastly, does excision of the ankle-joint afford as useful a limb to the working man as either Syme's or Pirogoff's operation? or does the removal of the malleoli and the consequent destruction of the mortice-joint so weaken the part and so predispose it to secondary mischief as to render the foot an encumbrance rather than a benefit? In the first case upon which I operated the boy played at leap-frog within twelve months after the operation; there was very slight shortening of the limb—not more than a moderate addition to the sole of his boot could rectify. In the second, performed Aug. 10th, 1855, upon a man aged twenty-five, I frequently see the man walking up the steep hill to Hampstead (a great strain even upon a sound ankle-joint) apparently with great ease. Whilst in the last case, which has just left the hospital, the boy walks about without difficulty. It is true that, as a matter of precaution, I have advised artificial support in the shape of upright springs inserted on either side of the boot.

But what takes place in Pirogoff's operation occurs equally in this: there is either an ingrowing of bone into bone, of the tibia into the astragalus or calcis, or so dense a ligamentous union as at once to afford the requisite strength and a certain amount of motion in the part. In the secondary amputation performed by Mr. Price, it was found that a considerable quantity of bone was thrown out from the lower end of the tibia, but that anchylosis was not complete. In Statham's case, amputated by Mr. Bowman, the under surface of the tibia was united by firm ligament to the tarsal bones.

In my first case, where complete anchylosis had apparently taken place, the mobility of the joints between the calcis and astragalus with the cuboid and navicular increased so much as to afford ample compensation.

It may also be imagined that the part is rendered still weaker and the movements of the foot more impaired by the displacement of the tibialis posticus and extensor tendons from behind the inner, as well as the peronei tendons from behind the external, malleolus; but there does not seem to be any inconvenience from this cause. I have never as yet had the opportunity of dissecting a leg which had been subjected to this operation; but from what I have observed I have every confidence that these defects are remedied in a much more efficient manner than is usually supposed, or at all events stated by authors; indeed, I know few more beautiful processes than that by which Nature rectifies the displacement or detachment of tendons. There is a preparation in the museum of the College of Surgeons of a man's foot who had been the subject of dislocation of the tibia with the astragalus and fibula inwards. The peronei tendons had been displaced from the groove behind the external malleolus; but to rectify this a hook-like process has been thrown out from the back of the malleolus to encircle the tendon and be a substitute for its natural groove. Again, I have seen two instances in which the adductor longus muscle was detached from the body of the pubis. In both the accident had occurred whilst hunting, and had been neglected, so that I did not see them for several months afterwards. In both I found a process of bone thrown out from the pubis, in one instance nearly two inches in length, extending downwards, and giving attachment to the displaced tendon, compensating as it were for the diminished length of the muscle. What has taken place in these cases I have every confidence takes place equally after excision of the ankle-joint, so that I have not the least doubt of the operation for excision of the ankle-joint affording as useful a limb to the poor man as to the rich, and a far more useful and comfortable limb than any which can possibly be supplied by either Syme's or Pirogoff's operation.—*Lancet, Jan.* 31, *and Feb.* 7, 1863, *pp.* 113, 142.

41.—CASE OF PERFECT BONY UNION OF A FRACTURED FEMUR, AFTER DEFYING ALL THE TREATMENT FOR NINETEEN MONTHS.

By E. M. James, Esq., Resident Surgeon, Melbourne Hospital.

John Dunston, ætatis twenty-two, miner, four years resident in the colony, accidently broke his thigh by a tree falling suddenly upon him, in the month of November, 1860; was in excellent health at that time. He states:—"A doctor set my limb immediately, and attended on me for three months. Finding at the end of this time the bone had not united, I went into the hospital at Castlemaine, where I remained under treatment for two months, with no better result." He then came to Melbourne, and was admitted into Melbourne Hospital on the 31st of May, 1861, under the care of Mr. Barker. On examination, his health was found to be excellent; no emaciation had resulted from his long confinement. The limb was shortened, but extension at once brought it to the natural length. No union of *any kind* had taken place; the fractured end could be felt rounded, and somewhat diminished, rather than increased in thickness. It was possible, with ease, to place the leg and lower fragment of the femur at right angles with the upper; the fracture was situated at the point of junction between the middle and lower thirds.

On June 18th, having been put under the influence of chloroform, a bistoury was passed down to the fracture on both sides; a steel spear was then introduced, and the ends of the bone well scraped; a piece of dry lint was placed over the wounds, and a straight splint at once applied to the back of the limb, from the trochanter down to the lower third of the leg. This being done, the long splint was applied in the usual manner, extending from the axilla to three inches beyond the foot; the bandage was starched, and the patient put to bed. No pain or inconvenience of any kind resulted from this proceeding, consequently nothing was touched till the 23rd of July, 1861, when the splints were taken off. On examination, the wounds had closed by first intention, but not the least attempt at union had taken place. There was obviously not a particle of ossific exudation. Feeling a little stiff and sore about the back, Mr. Barker ordered him to get up and use his crutches, only a short thigh splint to be applied to keep the limb steady.

August 15th. Being again put under the influence of chloroform, a straight incision, four inches in length, was made on the outside, over the fracture; the end of the upper fragment was then forced through the wound, and cut off by Hey's saw, to the extent of three quarters of an inch. Great efforts were then made to cut off the lower end in the same manner, but it was found impossible to get it out through the wound; consequently, the end had to be removed *in situ* by the chain saw. The limb was immediately placed on a straight splint as before, leaving the wound exposed. A long *interrupted* was then applied, from the axilla to three inches below the foot, but *no extension* was made.

Slight irritative fever followed the operation for three days. On the third day, the wet lint dressing was removed, and the centre of the wound was found united by the first intention. It was redressed, and a drip of lead lotion continued for three days, when it was changed for poultice of linseed meal.

At the end of eight weeks, the whole appliances was removed. On examination, no union had taken place; and little, if any, callus was thrown out, since there was free movement between the bones. The wound was all but closed.

A starch bandage was applied, fixing the hip, and a little opening was left to dress the wound. This remained for eight weeks more, but with no better result. The starch splint was thereupon removed, and three blisters were applied in succession, followed up by long splint for a month, but again without benefit.

He was then put up in starch and thin paste-board, fixing the hip as before, but not the foot, and ordered the following: Acid. phosphoric. dil. ♏xx.; infusi quassiæ ad ℥i. M. ft. haustus ter in die sumendus. He took coincidently aqua calcis as a drink.

At the end of eight weeks the starch splints were removed, and there appeared to be a *little* union, though lateral movement could still be felt. The limb was put up again for eight weeks more. On removing the splint at the end of this time, firm union had taken place, but not sufficient to admit of the splint being left off. The splints were then re-applied, and the mixture and lime water continued. He was permitted to go about on crutches.

At the end of six weeks the patient was discharged with a sound limb, and what was still more remarkable, with only one inch and a half of shortening, and in most unexceptionable general health, which indeed had not suffered any impairment during the long period of his confinement to bed (three-fourths of the whole time), and notwithstanding the almost complete deprivation of exercise. Assimilation and nutrition had, in fact, gone on regardless of the conditions adverse to their normal continuance.—*Australian Med. Journal, Oct.* 1862, *p.* 268.

42.—ON THE WIRE COMPRESS: A SUBSTITUTE FOR THE LIGATURE.

By JOHN DIX, Esq., Surgeon to the Hull and Sculcoates Dispensary.

(Read before the Royal Medical and Chirurgical Society.)

The wire compress—the subject of this paper—is a modification of the method of arresting hemorrhage devised by Dr. Simpson of Edinburgh, and introduced by him about three years ago as a substitute for the ligature. This "acupressure," as it is called, has been tested by but few surgeons of note; and in London, especially, it is almost unknown and ignored. Although probably a real improvement on the ligature, it undoubtedly labours under certain inherent disadvantages, most or all of which (it is believed) are obviated by the use of a fine wire of iron or of silver, instead of the steel needles of Dr. Simpson.

This idea was first promulgated in a paper on Acupressure published in the Medical Times and Gazette of June 2nd, 1860; and first put to the proof in a case of amputation of the finger, September, 1860. In this operation two arteries were secured by wire, which was removed on the third day. The case did well: there was no bleeding, and very slight suppuration.

In the next case—Chopart's amputation, performed April 26th, 1861—five wires were applied on as many arteries: four of these were removed in forty-eight hours, and the other on the fourth day. It was found that the wire was easily applied, as easily withdrawn, and entirely effectual for the purpose it was intended to serve—namely, the arrest of the bleeding from the cut vessels. This patient, being the subject of constitutional syphilis, did badly. There was sloughing of the entire surface of the wound, and the flap was totally destroyed; notwithstanding which there was no hemorrhage; but she died on the thirteenth day after the operation, of pyæmia.

Case 3, (Sept. 21st, 1861.)—In an amputation of the thigh, done after Mr. Luke's method, there were seven bleeding arteries. Upon five of these the wire was used, and with the femoral artery the femoral vein was intentionally included; two very small branches were treated by torsion. This case did well. Seventy two hours after the operation four of the "presse-artēre" wires were withdrawn with perfect ease and without bleeding. The one on the femoral remained five days, when it, too, was removed without any difficulty and without a trace of blood. There was but little suppuration, and an excellent stump was the ultimate result.

These cases prove that this mode of securing arteries is practicable, efficient, safe, and manageable. It is also believed to possess a certain positive superiority over the ligature, as the following comparison shows:—

A ligature in a wound impedes union and induces suppuration. Cure, by primary adhesion, of a large wound—as, for instance, an amputation—is an event of extreme rarity, and this because of the ligatures. A thread of silk is, in fact, a miniature seton, and the whole number required in an operation make up one of considerable size, and can scarcely fail to lead to the formation of pus. Again, the ligature of necessity excites ulceration of the artery upon which it is tied; it cannot in any other way be got rid of. This is another unhealthy process, antagonistic of repair. In applying a ligature, the end of the artery is drawn out from its sheath, by which its natural connexions are disturbed and its vasa vasorum broken up; its coats also are lacerated and bruised. The ligature remains for an indefinite time, long after it is useful or necessary, and it is not unfrequently pulled at by the dresser before it has become detached. Its knot, often deeply buried between the flaps, cannot be withdrawn without tearing through adhesions, or damaging the granulations. All these are serious obstacles to the healing process both in the stump and the artery itself, and much protract the period of cure. Moreover, the following is an interesting and noteworthy formula: Pyæmia is the offspring of purulent secretion, of which the ligature is an efficient and probable cause. Bleeding arises solely from ulceration of an artery, of which again the *primum mobile* is the ligature.

From one and all of these objections to the ligature the "wire compress" is almost or altogether free. Thus, in accordance with a well-known pathological law, *it*, being a metallic substance, is freely tolerated by the living body, and has little or no tendency to excite suppuration or irritation. Neither does it cause ulceration of the artery. This is positively affirmed from actual observation of its effect as witnessed in the sloughing stump before alluded to. It is applied without interference with the natural relations and vital connexions of the vessel. It is removed at any time, according to the will and judgment of the surgeon, without disturbance to the reparative action going on in the artery and in the rest of the wound, without futile premature attempts, and almost without pain to the patient. It is not liable to lose its hold, or to become detached too soon, as not unfrequently happens to a ligature applied upon a brittle or sloughing artery. Twigs of nerve accidentally included in the embrace of the wire are not injured and excited as by the tight strangulation of the ligature, and, if thought advisable, the veins are easily and safely occluded, along with the arteries.

Although this has been spoken of merely as a modification of acupressure, yet it is believed to be a decided and important improvement on "Simpson's skewers," as the needles have been irreverently called, and which are fairly open to the following objections. When several of them are required, the stump resents, as it were, being thus pierced through and through in various directions. From the injury thus inflicted, and from the obstruction to the capillary circulation caused by the pressure of the unyielding steel, arise much tension, œdematous swelling, and great pain; the pain, especially, has been found a very serious evil. Again, their projecting ends, and the puckering they cause in the substance of the flaps, interfere very much with that accurate adjustment of the cut surfaces and edges which so greatly aids the chances of union by adhesion.

The wire is free from all these shortcomings. It is thus applied :—Take a piece of surgical wire six or eight inches long, and thread each end thereof upon a straight needle. Seize the bleeding mouth of the artery with forceps, and pass one of the aforesaid needles close on each side of the artery just mentioned, about a line above the points of the forceps, directly down through the substance of the flaps, so that they emerge at the cuticular surface, about half an inch distant from each other. Draw them both through together till the curve of the wire compresses the artery on the face of the flap. Now get rid of the needles by clipping through the wire close above their eyes, and also detach the artery forceps. Place a piece of cork, cut for the occasion, upon the skin, between the points of exit of the wire, and over this twist the wire tighter and tighter until the bleeding is arrested. Lastly, cut off the superfluous wire. All which is done quicker than described. Two or more arteries lying near together may be embraced by one wire, and, as has been said, the veins may be included or excluded at will.

The wire should be either of silver, or, what is much cheaper and equally manageable, of the finest and softest passive iron. The generality of wire as used for sutures is too hard and stiff. The needles are about 3 in. in length, straight, and three-edged, with an eye adapted for carrying wire. Special care is necessary in threading the wire that it is kept perfectly free from all twisting. The forceps are used not to draw out the artery as when a ligature has to be applied (this, indeed, is to be particularly avoided), but merely as a guide to mark the exact position and course of the vessel. The cork is necessary to protect the skin from the pressure of the wire.

The withdrawal of the wire, which at first sight appears an insuperable difficulty, is perfectly simple and easy. It is thus effected :—Clip the wire close to the edge of the piece of cork, and straighten out the curve it has formed, at its exit from the skin. Remove the cork, and apply instead the tip of one finger, with which press firmly upon the flap, making traction gently and gradually upon the other end of the wire. If this were roughly and hastily done, it might break up the adhesion which it is presumed has taken place between the surfaces of the flaps, and it is quite possible that a flexure in the wire might lacerate the artery in passing over it; but it is certain that none of these evils need happen with ordinary care and tact.

As to the period of withdrawal, further observations are desirable; but it has been clearly shown in numerous cases of acupressure, that for small vessels a few hours of compression is sufficient, and for the largest arteries a much less time than might be supposed. However, as a general rule, it is not desirable to disturb a wound in any way for from twenty-four to forty-eight hours; at the end of which time all wires commanding the secondary branches may be safely removed, and probably also from the large arteries; but, as a matter of prudence it is well to keep a check upon such a one as the femoral for three or four days at least.

The descriptions hitherto given apply particularly to amputations; but the wire is equally applicable to many other operations.

In a postscript to this paper was related a case of excision of the testicle, in which it was used most satisfactorily upon the vessels of the cord. Two wires were required, and they were removed on the fourth day. Also a case of excision of the breast in which three wires were applied, and removed in twenty-four hours. The wound, which was six inches long healed kindly and rapidly—almost without suppuration.

Suppose the femoral artery needs to be secured for popliteal aneurism. The wire would be passed under the artery by means of a tubular aneurism-needle made for the purpose, brought out through the integument at a convenient situation, and then twisted upon a cork in the usual way. This same artery, cut in amputation, is securely closed in three or four days by the action of metallic pressure; consequently, in three or four days the wire might be removed. Meanwhile, it has not caused suppuration, or impeded the union of the wound, which ought, therefore, by this time to be completely healed; and more important still, the artery is not cut through, as by the ligature, but its coats remain intact, and bleeding is impossible.

It will probably be found that the pedicle in ovariotomy may be conveniently dealt with by this method, the arteries being secured individually, and the entire stump also fixed to the abdominal wall by another wire. This idea has been already promulgated by Mr. Spencer Wells.

The paper was illustrated by models.

Mr. Fergusson said that the author's paper ought not to pass without remark, as the subject was novel to the society, though not new to the surgical department of the profession. The paper was very interesting and very clever, and yet the materials of it were not sufficient to prove the author's point, as five cases only were brought forward. These, if the subject had been quite new, might have been enough. From the cases adduced, he had not learned that the compress had, in the smallest degree, any advantage over the ligature, which as a practical surgeon, he was not disposed to set aside. It had been first brought forward by a gentleman of great talents, but he (Mr. Fergusson) had not seen, nor heard, nor found anything to induce him to imagine this process superior to the ligature. The cases related were very few, whilst ten times that number of instances of the employment of other means might be brought forward. As regards the amputation of a finger, every one knew that such cases would do perfectly well with the ligature. In the case of partial amputation of the foot, he did not suppose that the sloughing was due to the wire; but the fact that there was no hemorrhage did not prove that the wire was better than the ordinary method of securing bleeding vessels. As to the plan adopted with regard to the thigh, that was not new. Professor Handyside had tried this method. The history of the case did not vary from the ordinary run of such amputations. He (Mr. Fergusson) had tried this plan repeatedly, but should require much greater proof of its value than he had yet heard before he should attempt to secure large arteries, as the femoral or the brachial, on this plan. He noticed that in this paper, as also in many others on the same subject, there was a constant attempt to decry the ligature, one of the grandest things in surgery. Great pains had been taken to test the value of the ligature, and he thought that the question had been set at rest. One point urged against it was, that it gave rise to suppuration, and thus favoured pyæmia. Of this he was doubtful, as he had often observed that patients, after operation, had feverish symptoms which gave rise to great anxiety, but which passed off as soon as suppuration set in. In fact, he always calculated on suppuration as relief to the patient. He thought that the tracts of the ligatures acted as vents for the discharge of pus; and as regards their preventing healing by first intention, he would say that the advantages of this kind of union were overrated. Suppose the stump did heal by first intention, it could not be used for weeks—for months, as it was in great measure new material, which would not bear pressure. In fact, the ligatures were of advantage in this way, that they prevented the patient using his limb too soon.—*Lancet, Jan.* 24, 1863, *p.* 94.

43.—THE EMPLOYMENT OF TELEGRAPH SUTURES IN CLOSING WOUNDS IN VARIOUS CASES.

Case under the care of J. Erichsen, Esq., at University College Hospital.

Mr. Clover has introduced a new form of suture, which appears to possess many advantages over those ordinarily employed, whether of silk, thread, or metal. It consists in the adoption of a very fine copper wire covered with gutta percha —in fact, a minute telegraphic wire, and therefore receiving the name of "telegraph sutures." We have seen them used in a number of cases at University College Hospital, wherein Mr. Clover was himself allowed to apply them, because, as we heard Mr. Erichsen remark, it was but fair to allow the inventor to show the application of his own invention.

One of the first cases which came under our notice was one in which Mr. Erichsen removed a tumour the size of an orange from the right parotid region of an elderly man on the 22nd of October last. The growth, although movable, was situated in a region requiring care to excise it, for it lay between the angle of the jaw and mastoid process, and did not dip behind the former. The tumour proved rather hard on making a section, and was fibro-nucleated in character—a form that is considered to be somewhat rare. The incision used was T-shaped, and this was closed with a fine telegraph wire, in the continuous or Glover's suture, by Mr. Clover; a small portion being left open to permit of the drain of secretion.

On the 17th inst., the edges of a double hare-lip in an infant were pared by Mr. Erichsen, and evenly brought together by the deep interrupted telegraph sutures by Mr. Clover. These, the former gentleman remarked, leave no scar—nothing at all like the hare-lip pin. He further stated that he had used them on the face of a gentleman from whom he had removed a tumour ten days previously, and no cicatrix was visible. He had also employed them in other cases. The silver wire, he observed, had the disadvantage of being very rigid, and does not tie well. The telegraph wire, on the contrary, is quite soft, and forms a knot like ordinary silk thread; it can be cut like silk, and be as readily taken out; it can, moreover, be used with a fine sewing needle.—*Lancet, Dec.* 27, 1862, *p.* 700.

44.—ON SILVER-WIRE LIGATURES.

By Redfern Davies, Esq., Birmingham.

[Having three years ago to treat a bad case of elephantiasis of the leg, Mr. Davies tied the popliteal artery, using a silver wire for a ligature. This plan of treatment was first proposed and carried out by Dr. Carnocan. The wire came away on the twenty-first day—the result of the operation being an almost complete diminution of the size of the limb to its natural dimensions.]

The best, most concise, and accurate description with which I am acquainted of the effects produced upon an artery by its ligation is in Mr. Guthrie's "Commentaries on the Surgery of the War." He says: "When a small and round ligature is properly applied to an artery of large size, such as the femoral, the sides of the vessel are brought together in a plaited, folded, or wrinkled manner; the ancient middle and inner coats of the artery, including the modern four, are divided, while the outer one remains entire and apparently unhurt. If the ligature be removed an impression or indentation made by it on the artery will remain as a mark; and if the artery be slit open in a careful manner the division of the inner coats will be obvious." And again he says: "The inner and middle coats, formed by four distinct layers or structures, are not only divided, but, the inner ones particularly seem to be curled inwards on themselves, so that the cut edge of one side, or half, is not applied to its fellow in the usual way of two surfaces, but, by curling inwards, meets its opponent on every point of a circle, and in this way forms a barrier inside that of the external coat, which is tied around it by the ligature; so that, in fact, when a small ligature is firmly tied, its direct pressure is not applied to the inner coats, which have been divided and curled away from it, but to the two layers of the outer coat, which are, in consequence of that pressure, made to ulcerate or slough. The cut edges of the four inner layers, being from this provision of nature perfectly free, are capable of taking on the process of inflammation, which stops at the adhesive stage. This they do by the effusion of lymph or fibrin, both within and without, to a greater or less extent as the case may require."

I have frequently tested the truthfulness of this description by examining arteries after they have been tied with silk, both upon the dead and the living subject, and have occasionally had opportunities of doing so after a ligation has been made with a silver wire. The result of my observations has been that their effects were identical, and that they corresponded with Guthrie's account.

In October 24th, 1862, a patient of Dr. Porter, upon whom I had tied the arteries with silver wire after an amputation of the leg four days previously, died. Within the posterior tibial artery—the only artery I was enabled to examine—I found that for half an inch above the site of ligation, the blood was firmly coagulated, that the internal and middle coats of the artery were uniformly cut through, and that there was as completely organized a structure as could be expected in so short a time between and around them.

Allowing, then, that a ligature of silver wire acts as well as does one of silk, is any advantage to be ascribed to it? or is it a mere change of the material constituting the ligature? I believe that there is a material advantage in a silver-wire ligature over the usual silken one; and that this advantage consists in the capability of silver-wire to remain in tissues nearly if not quite innocuous. Thus, while the artery is undergoing the process of obliteration, a silver-wire ligature does not of itself constitute a focus for purulent secretion, permeating tissues which we are endeavouring to unite by immediate adhesion, and these tissues, temporarily weakened by its supply of blood, being diminished.

Of the harmlessness of silver-wire in living tissue I have long been convinced. In a girl upon whom I operated for staphyloraphy nearly four years ago a silver-wire still remains; it is the lowest one of three sutures that I inserted. At the time of the removal of the other two, not being satisfied with the union of the soft palate, I permitted it to remain, and was not allowed afterwards to remove it, as it occasioned no inconvenience.

About two years ago I operated on a case of vesico-vaginal fistula by simply uniting the avivified edges of the fistula with silver-wire sutures. On the tenth day of their insertion I intended to remove them, but, from an accident, was unable to do so for three months, when I found the case perfectly cured.

Elsewhere I have recorded and illustrated by a woodcut of the parts implicated a case in which I had attempted a radical cure of hernia upon a woman. I had operated by the method described in Druitt's Surgery, as one of my *procédés.* The patient, on the eighth day after operation, died from the consequences of intussusception of the bowel on the opposite side to the one I had operated upon. The parts implicated in the operative procedure demonstrated well the perfect immunity from inflammatory process with which the silver-wire had perforated the peritoneum even in three places. In a case which I published last year in the Lancet, I say: "A patient, aged seventeen years, was admitted into the Birmingham Workhouse Infirmary on the 18th of December, 1858. He was the subject of varicocele on the left side, which had existed for three years, and to so great an extent as to prevent him from following his employment. Being desirous of radically curing him, I adopted the method of M. Ricord, which I had seen him perform with success; but instead of using the silken ligature which he used, I tried to do the same thing with silver-wire—namely, between the vas deferens and the veins. I passed, by means of a needle, a double wire, and by the same apertures in the opposite direction, anterior to all the veins, another wire. By engaging one free end through the loop of the other one on both sides, the veins were, by traction on the wire, compressed. This traction was kept up continuously by means of an elastic watch spring, bent in the shape of a horse-shoe. In ten days' time, thinking the varicocele cured, and wishing to remove the wire, I tried to do so, and found I could not, though it was easy enough with silken ligatures. I therefore cut off the wires as short as I could, trusting to the innocuity of silver-wire to cause no harm. In another week the punctures were well healed, and the patient was discharged, the varicocele then appearing perfectly cured. Seeing him again in about a year's time, I found upon examination, that all things remained as when he left, and he stated that he had resumed his work immediately upon leaving. He himself was totally unaware of there being any wires in his scrotum."

But as a wire of silver is a foreign body to the tissues, it should, therefore, be treated as such, removed in general, and left alone when its removal is undesirable in any particular case.

In conclusion I would say, that when left temporarily in the soft parts, as a ligature, I have observed that it creates a surprisingly *small amount* of irritation in them, and am very glad that the same fact has been similarly noticed and spoken of to me by those who have had the fullest opportunities of noticing the effects produced by silver-wire ligatures in cases of amputation under their care.—*Lancet, Feb.* 28, 1863, *p.* 233

45.—HORSEHAIR AS A SUBSTITUTE FOR WIRE.

By THOMAS SMITH, Esq., Demonstrator of Anatomy at St. Bartholomew's Hospital, and Assistant-Surgeon to the Hospital for Sick Children.

Experience has ere this fully justified the claims put forward by Dr. Marion Sims, Dr. Simpson, and others, for the advantages of the metallic suture over threads of organic origin. For ordinary purposes, the cleanliness, security, and unirritating nature of silver wire leave but little to be desired in the way of improvement in material; and though in most cases such sutures answer all purposes for which they are designed, yet to some localities and to certain tissues they are not well suited. The adjustment of wire sutures to wounds of the eye-lid, foreskin, scrotum, and other parts where the integuments are lax and delicate in texture, is difficult, and their withdrawal is painful and liable to cause rending and laceration of the freshly united edges. The difficulty of introduction consists in the laxity of the parts, disposing the edges of the wound to twist and coil up far more readily than ends of the wire; while the withdrawal of the sutures is painful, owing to the rigid condition the wire assumes before its removal, and this notwithstanding every precaution that may be taken to anneal it previous to its introduction. In allusion to this inconvenience, Dr. Mott, in his work "On Surgery," in speaking of metallic sutures, makes the remark: "The introduction of wire sutures is easy enough; the withdrawal is often attended with great inconvenience, and even with risk of tearing the imperfectly united edges of the wound asunder." (Vol. i., p. 335.)

With a view of finding a material for sutures as unirritating and as unabsorbent as wire, but more easy of adjustment and withdrawal, I performed during last spring a series of experiments on animals to determine the suitability of horsehair as a substitute for wire in certain cases. The horsehair used was such as is ordinarily sold by fishing-tackle makers. The experiments were performed upon dogs. The general results showed that there was no appreciable difference shown by the tissues in their tolerance of silver wire and horsehair. Both materials were equally unirritant; yet there was a difference in favour of horsehair in the greater facility of its adjustment and subsequent removal.

For the comparison between silk and horsehair as illustrating the relative merits of the two materials for sutures, I venture to refer to the following experiments :—

June 10th, 1861. Two wounds of equal length, dividing the entire thickness of the integuments, were made on opposite and corresponding parts of a dog's abdomen; four sutures were applied at equal intervals to each, horsehair being used to one wound, and fine ligature silk to the opposite. On the third day both wounds looked alike healthy, and having their edges in close contact. On the fifth day the edges of the wound with silk sutures were slightly reddened, and pouting a little between the points of suture; the opposite wound had united without suppuration. On the eighth day three out of the four silk stitches had cut their way out, and next day the remaining one came away, leaving the edges of the wound just separated, but granulating healthily. Three days later the wound had almost entirely healed. At this time the opposite wound had healed up soundly around the tracks of the horsehair sutures, which remained *in situ*, exciting no irritation whatever, until the dog's death, a month after the commencement of the experiment.

May 3rd. The opposite femoral arteries of a dog were exposed to the same extent just below Poupart's ligament. Around the vessel on the right lower limb was passed a stout horsehair, and loosely tied; a silk suture being similarly adjusted around the opposite artery. A month after the operation the wound on the right side was all but healed, and was secreting a little serous discharge. At the same time the wound on the left side was swollen, its edges were everted and inflamed, and there was a profuse sanio-purulent discharge. Two days later the wound on the right side had healed around the track of the horsehair seton, which was retained. While around the silk on the other side there was profuse suppuration; the surrounding parts were red, tender, and much swollen; and as the animal's general health was suffering, and it was rapidly emaciating, the silk was withdrawn. The wound now speedily altered its character, and by June 20th was soundly healed. September 3rd, four months after its introduction, the horsehair still remained around the right femoral artery, exciting no irritation, the parts being soundly healed around the track of the seton.

The unirritating nature of horsehair as a material for suture is no less marked when applied to the tissues of the human body. It was used by Mr. Paget in a case of double entropion, the wound of the operation being in one eyelid secured with horsehair sutures, while the opposite was brought together with fine sewing cotton. At the end of the week three out of the four cotton sutures had cut out, while at the same time all four horsehair sutures remained firm.

As a material for attaching the margins of the skin, and mucous membrane after circumcision, or other operations for phimosis, I have found horsehair most useful, having employed it both in children and adults. In one case particularly, where a complete circumcision of the foreskin, with a free division of the mucous membrane was performed on a middle-aged gentleman, its good effect was remarkable. Six sutures were introduced, and excited so little disturbance that the patient was not kept for a single day from his business, which involved pretty active exercise. The wound healed without suppuration, and though left in, at the patient's request, some of them for fourthen days, the sutures caused no irritation, and were removed at last without difficulty. In the removal, the advantage of horsehair sutures over wire is considerable, since, unlike wire, which after remaining a few days in a wound, stiffens into a metallic ring, horsehair, when cut just aside the knot, either retaining its original elasticity, springs open, or if it has been long soaked in the wound secretions, it becomes soft and pliable. I would recommend the use of this suture for wounds of the eyelid and other parts of the face, and to the loose integuments of the scrotum and penis; since to all these parts I have either applied the suture myself with good effect, or I have seen it used by others at my suggestion.

But I can imagine that there are other uses to which it might be extended, and especially to facilitate the union of wounds of the conjunctivæ. For the purposes of suture, long white tail hairs are the best. Before being used they should be soaked for a minute or two in water, or they may be drawn once or twice through the moistened finger-ends. The suture may be fastened off in a double knot, but if the hair is stiff, a third knot is often required. It may be removed in the ordinary manner, seizing the knot with the forceps, and dividing the suture just aside of it. It is scarcely necessary to remark, that horsehair, as a suture, is not suitable for wounds where there is much tension between the edges.—*Lancet, Nov.* 8, 1862, *p.* 501.

46.—PRESENCE OF BALLS IN WOUNDS.

By M. Nelaton.

[The following is from the clinical remarks made at Paris, by M. Nelaton, on the case of Garibaldi.]

M. E. Rousseau gave me the idea of an instrument which would bring away particles of metal. We had, therefore, a probe made, to the end of which was fixed an olive-shaped body, made of unpolished china, upon which mere contact would leave a mark of the projectile, and prove its presence. M. Charrière made several instruments of this kind, composed of a silver stem, ending in a china knob. The mere rubbing of this instrument against lead is sufficient to make a stain, which neither the soft parts nor the morbid secretions could obliterate; a simple rotation of the instrument upon its axis fixes upon it a kind of meridian.—*Lancet, Dec.* 6, 1862, *p.* 631.

47.—FAVRE'S GALVANIC PROBE FOR DISCOVERING THE PRESENCE OF A BALL OR OTHER METALLIC BODY WITHIN A WOUND.

[The preceding article contains an account of Nelaton's porcelain probe, by means of which the presence of a ball in Garibaldi's wound was positively determined. The following is another contrivance for detecting metallic bodies in wounds, more complicated but not less ingenious. The credit is due to the fertile brain of M. Favre, Professor of Chemistry to the Faculty of Sciences of Marseilles.]

The principle involved is the different conducting power for electricity between a metallic substance, on the one hand, and the fluids and tissues of the body, on the other. Thus it happens, that if the conducting wire of a feeble galvanic circuit connected with a galvanometer be broken, and a bullet or other metallic body be interposed, on the completion of the circuit the needle will be strongly deflected. This would not be the case on interposing the ordinary tissues of the body. With the assistance of M. Favre, Dr. Fontan, a military surgeon at Marseilles, has made experiments to test the efficiency of the plan proposed by the former. The apparatus used was as follows:—1. A Smee's battery of a single pair of plates, so feeble as to be unable to produce any sensible amount of electrolysis with the animal tissues. 2. A galvanometer. 3. A probe, formed in the following manner: it consists of two parts, one of which is received into the other, which forms thus an outer casing. The internal portion, or galvanic portion, is a straight metallic probe, through the length of which pass, packed in silk, two metallic wires, each terminating in a steel needle, and isolated and firmly fixed beyond the extremity of the probe by means of a little cone of ivory. These wires are intended to conduct the galvanic current, the galvanometer being placed in the course of the intended circuit. For this inner probe, there is provided an external metallic casing, so constructed as to permit the galvanic probe to be slid within it, and perforated at its extremity with two holes, through which the needle-points of the conducting wires can be protruded. There are various little arrangements of grooves, slits, and a screw to ensure accuracy of adaptation between the galvanic probe and its case. In using the instrument, the internal probe is retracted and fixed by the little screw, and the instrument is then employed in the ordinary way as a probe. On the solid substance being arrived at, the screw is loosened, the needle points protruded, and brought in contact with it, by sliding down the internal probe. If the substance be metallic the circuit is established, and the galvanometer deflected. The first experiments were made with wires unprovided with needle-points, the addition of which were found essential to success, inasmuch as they readily penetrated any soft tissues or adventitious substances, pieces of clothing, leather, &c., which might chance to be interposed, and also overcame the difficulty likely to result from oxidation of the surface of the metallic substance, &c. The use of a little friction with the point of the sound, or a gentle percussion upon it, never failed to establish the galvanic current. M. Fontan suggests that, by modification of this probe, the principle involved might be applied to other purposes of diagnosis, as where the presence of metallic bodies in the canals of mucous cavities of the body was to be detected—*Medical Times and Gazette, Dec.* 13, 1862, *p.* 641.

ORGANS OF CIRCULATION.

48.—SUCCESSFUL USE OF IODIDE OF POTASSIUM IN THE TREATMENT OF ANEURISM.

By Dr. William Roberts, Physician to the Manchester Royal Infirmary.

[The patient a collier, aged thirty-nine, was admitted with symptoms indicating thoracic aneurism, dating from a kick on the chest whilst in a state of intoxication four months previously. The superficial veins of the upper arms and the external jugulars were somewhat distended; the right radial pulse was slightly feebler than the left.]

The pupil of the left eye was motionless from previous accident, the right pupil was natural. There was some dysphagia—when the chest was uncovered, the first bone of the sternum and its immediate vicinities were observed to be bulged out, and to be the seat of a heaving pulsation. On the left side of this prominence, in the second intercostal space, close to the sternum, there was a soft pulsating elevation standing out about a quarter of an inch above the surrounding level. This elevation was conical in shape, and had a base about the size of a shilling. The heart's apex beat in the fifth interspace within the nipple line.

Percussion revealed a considerable area of diminished resonance over the bulging parts. Transversely, in the level of the second interspaces, the dulness measured four inches and a half, extending more to the left than the right of middle line ; vertically, there was dulness for three inches and a half below the sternal notch. There was no fulness or pulsation in the last named spot. The cardiac sounds were normal over the precordial region; they were louldly audible over the seat of dulness above described.

The first sound was faintly murmurish over the soft elevation; and a distinct though faint systolic murmur existed in the course of the innominate artery; this was intensified over the right carotid and in the acromial angle. No murmur existed over the left carotid and subclavian. With such an array of symptoms and physical signs, the diagnosis could not remain for a moment doubtful. The case was one of aneurism of the arch of the aorta making its way forward through the parietes of the chest.

On October 10th, the patient was ordered to take five grains of iodide of potassium three times a day; to keep his bed ; and to restrain as much as possible the quantity of liquids taken in the way of drink. Three days later the iodide was increased to seven grains and a half three times a day. Six days after the commencement of the treatment, the patient reported himself a great deal better; the paroxysms of pain no longer recurred; the cough and difficulty of breathing were less troublesome; and the soft pulsating elevation was slightly less prominent. The physical signs remained otherwise unchanged. The iodide was now increased to ten grains three times a day.

From this date, October 16th, to October 27th (a period of eleven days) the patient went on favourably. He continued free from pain, difficulty of breathing, and dysphagia; and the soft elevation had almost altogether subsided to the level of the surrounding parts.

He was now allowed to get up, and the restrictions as to fluids were relaxed. The iodide was increased to fifteen grains three times a day.

On November 4th (twenty-fifth day of treatment), the dulness measured four inches across, and two inches and three-quarters vertically. The dose was now raised to twenty grains.

On November 18th (thirty-ninth day of treatment), the area of dulness was carefully measured again. It had now contracted to three inches and three-quarters transversely, and two inches and a half vertically; so that from the commencement of the treatment, it had diminished three-quarters of an inch in one direction, and one inch in the other. The soft elevation had sunk to the level of the surrounding parts, and had become wholly inappreciable to the eye. Pulsation was still felt over the spot. The patient was free from pain and difficulty of breathing, and felt himself exceedingly well. The cough was still stridulous in character, and the veins of the neck and upper arms were not perceptibly less distended. The inequality of the radial pulses had not disappeared.

This is the sum of the improvement after a treatment of six weeks; and it is certainly a striking result in so unmanageable a disorder as a protruding aneurism of the arch of the aorta.

[Since the delivery of this lecture the patient has continued the use of the iodide of potassium up to the present time (Dec. 23). An obstinate diarrhœa necessitated the diminution of the iodide to five grains three times a day. The bulging of the first bone of the sternum is conspicuously less, and the limits of dulness correspond to the following dimensions: transversely, three inches and a half; vertically, two inches and a quarter.

The state of the radial pulses, and of the superficial veins of the neck and arms, together with the character of the voice, remain unaltered; and there is decided emaciation.]

I will now relate to you the history of two other cases, within my knowledge, of aneurism treated with iodide of potassium.

About a twelvemonth ago, I saw in consultation with Mr. T. Windsor, who first called my attention to this mode of treating aneurism, a lady, twenty-nine years of age, suffering from an undoubted aneurism of the arch of the aorta, implicating the commencement of the arteria innominata. There were violent pains on the right side of the head, paroxysmal dyspnœa, excessive pulsation at the root of the neck on the right side, and sleeplessness. A dull space extended from a little to the inside of the right sterno-clavicular articulation for a distance of two inches outward. Over this space there was a heaving pulsation. No murmur existed with either sound; but the sounds were intensified over the arch, and towards the clavicle. Repeated slight hæmoptysis had been observed.

For an account of the further progress of the case, and the treatment, I am indebted to Mr. Windsor.

In the succeeding six months the condition of the patient became steadily aggravated in spite of a persevering observance of the recumbent posture and a regulated diet. On several occasions the voice was temporarily lost; in addition to some dysphagia, there was constant troublesome cough, with tenacious scanty expectoration, and occasionally severe paroxysms. The difficulty of breathing and pains were often so severe, that the patient was forced to quit her bed, and to walk about. She had become considerably emaciated, and so weak as to be unable to leave her room. The clavicle also began to project, and at length stood out half an inch beyond its natural level. The pulsation at the root of the neck increased; and the right eye became the seat of a severe congestion.

The iodide of potassium was first administered in April last in the hope of diminishing the pains in the head. The desired effect was speedily produced; and the medicine was discontinued after it had been used a week. In July, the patient growing daily worse, and a fatal termination appearing not to be very far off, Mr. Windsor again administered iodide of potassium in doses of five grains three times a day. This quantity was shortly after increased to ten grains; but owing to the occurrence of violent salivation, the dose was again brought down to five grains. This quantity agreed; and the patient has been taking it until the present time, with great benefit, as you shall hear. The general symptoms subsided quickly; the cough, pain, dyspnœa, and dysphagia, disappeared; the occasional hæmoptysis ceased to recur; the patient gained flesh and strength, and two months ago she was able to walk six miles; and, more important than all, the projection of the clavicle progressively receded, until it has now almost gone back to its natural position.

The third case which has come under my personal notice is a patient of my colleague, Dr. Wilkinson.

This man was admitted into the infirmary on October 20th, with a thoracic aneurism coming forward in the neck. In a few days, it formed a tumour as large as a child's head, extending on the left side from the clavicle almost to the angle of the jaw, causing very great distress; and passing on to what appeared a speedy termination by rupture. This man was put under gradually increasing doses of the iodide of potassium until the quantity attained was fifteen grains three times a day. No positive amelioration can be said to have taken place in the condition of the aneurism; but the distress is scarcely so great as it was, and the rapid growth of the tumour seems to have been stayed.

[Seventeen days after the delivery of the lecture, Dr. Wilkinson's patient died. Death took place from pressure of the tumour on the left pneumogastric nerve, and consequent pulmonary mischief. The vagus trunk was flattened out and atrophied for the space of an inch and a half in the neck. Only a few nerve-tubules could be discovered in this part by the microscope. The left lung was the seat of several patches of gangrene as large as hen's eggs, surrounded by areas of consolidation. The pulmonary gangrene had been detected a week before death. The sac of the aneurism was lined with thick layers of coagulated fibrine; and a large firm blanched clot attached by a somewhat broad pedicle to the upper parts of the parietes, floated freely in the sac.]

So far as I know, the first notice of the use of iodide of potassium in aneurism occurs in a clinical lecture of Professor Nélaton (*Clinique Européenne*, July, 1859). M. Nélaton had been consulted by a Pole on account of a tumour in the lower part of the neck. Several physicians of eminence, Bouillaud, Andral, Beau, had previously seen the case, and had diagnosticated an aneurism of the innominate artery or of the aorta. The patient stated that while residing in Warsaw, iodide of potassium had been administered to him with great relief. Nélaton took the hint, and prescribed the same. To his astonishment, a very notable amelioration followed, which went on almost to complete disappearance of the tumour, and the patient returned to his country in a satisfactory state of health.

In a late number of the same journal (Aug. 27th, 1859), you may find a very brief account of a number of cases by Bouillaud. One was a woman with aneurism of the carotid artery. Iodide of potassium was administered in *gramme* doses; then in two *gramme* doses for two months. At the end of that time, the tumour, previously as large as a pigeon's egg, had diminished almost to disappearance. The second was an aneurism of the aorta and innominate in a man. The tumour, which had a very considerable volume, had suffered displacement and very great diminution of size under iodide of potassium. The case was under treatment when reported.

A third instance related to a large aneurism at the point of origin of the carotid and subclavian. The tumour had diminished considerably in a few weeks under the iodide.

A colonel with an aneurism of the carotid furnishes a fourth case in which Bouillaud saw the tumour almost wholly disappear after taking the same drug.

The next reference to the subject comes from a wholly independent source, and is, for that reason, of additional value. It occurs in a paper by Dr. Chuckerbutty of Calcutta, published in the British Medical Journal for July, 1862. He gives an account of four cases.

The first of these was an aneurism of the inominate artery growing steadily upward into the neck. The growth ceased on the administration of iodide of potassium in decoction of cinchona, and the sac gradually solidified. The patient however, was carried off, some months afterwards, by an attack of bronchitis. The autopsy revealed a sac as large as a pear, filled with dense coagula, leaving merely a narrow channel on the outer aspect, through which the right carotid and subclavian arteries communicated with the aorta.

In a second case, marked temporary relief followed the administration of the iodide in an immense aneurism of the aorta. The aneurism finally destroyed the patient by rupture. A *post mortem* examination three hours after death showed the sac to be filled with dense solid coagula.

In a third case of large aortic aneurism, temporary amendment took place so far that the patient left the hospital, considering himself cured. Three months later, he returned, with intense aggravation of all his symptoms, and died in a few days.

In the fourth example, an aortic aneurism which formed a a dome-shaped tumour two inches in diameter, coming forward through the sternum, had been treated in vain by turpentine, sulphuric acid, and other remedies. After the administration of iodide of potassium internally, and the application of the tincture of iodine externally, the patient steadily improved in every respect; the thoracic pain disappeared, the swelling diminished, the hæmoptysis ceased, and the patient ate and slept like any other man. He was still under treatment.

I do not wish to make too much of these facts. The erratic course of aneurismal tumours, and the occasional occurrence of spontaneous consolidation of their sacs are well known. Nevertheless, the twelve cases here brought together form a very remarkable series. In all of them, save one, striking relief of suffering followed the use of the drug; in eight, an undoubted diminution of the size of the sac took place; and in a few, complete subsidence of the swelling seems to have occurred.

The cases of Dr. Chuckerbutty would appear to indicate that the beneficial effect of the iodide was owing to its power (hitherto wholly unsuspected) of increasing the coagulability of the blood. Dr. Wilkinson's case lends support to this view; for not only was the sac lined with layers of fibrine but a very firm, decolorised, fibrinous mass, attached on one side, floated in the cavity of the aneurism. The value of the above observations, as bearing on the conclusion inferred, is greatly increased by the circumstance that they are derived from two distinct and wholly independent sources.

Enough has certainly been advanced to encourage an inquiry into the power of iodide of potassium as a general hæmostatic; and to demand a fuller trial of its effects in internal and external aneurisms; in the treatment of which no internal remedy hitherto employed has succeeded in gaining even a temporary footing.—*British Medical Journal, Jan.* 24, 1863, *p.* 83.

49.—CASE OF OSTEO-ANEURISM.

By Dr. E. D. Mapother. (Case read before the Surgical Society of Ireland.)

Three months ago I was consulted by a gentleman, aged 28, concerning a tumour which had grown upon his left leg. He attributed it to having struck against a chair while walking through the drawing-room after dark about five years before. He had tried blisters, ointments, and other means for its removal, without success. It had progressed at a much more rapid rate for the month before I saw him. It was situated on the inner surface of the left tibia, about its centre, and appeared like a node, save that it was larger and more prominent, projecting externally to the size of a large walnut, and, as we had afterwards reason to believe, dipping into the bone for an equal depth. The skin was tightly stretched over it, but was not at all discoloured. It had a slight degree of elasticity, but its most remarkable features were a well-marked thrill or frémissement, and a distinct expansive pulsation, which latter was demonstrated by placing a bit of paper on the tumour and looking at it *en profile.* The pulsating mass rose up suddenly from the bone, which was plane and healthy around it. A slight bruit was audible with the stethoscope. Pressure on the femoral or popliteal arteries did not remove these symptoms. It was occasionally painful and tender. His general health was admirable, and there was no sign of strumous, syphilitic, or cancerous cachexia. As I stated that some operative procedure was called for, the family wished for the advice of Dr. Hutton, to whose mature judgment the successful issue of the case is mainly due. We determined first to remove the skin by potassa fusa, and this was done readily and rapidly by first raising the cuticle with a blister. The thrill, pulsation, and bruit were now found more evident, and the bone was seen to form a thin cribriform shell, the periosteum still covering the small spots where it had been removed. The tumour did not give the sensation similar to that experienced in squeezing parchment or egg-shell, which Chelius considers very characteristic. Remembering the frequent occurrence of phlebitis when the veins of bone are divided, we were disinclined to remove the tumour by a gouge or other cutting instrument. I may here say incidentally that I have always regarded the ready absorbtion of pus, and occurrence of phlebitis consequent upon injury of bone, as due to the peculiar anatomical arrangement of its veins. As described many years ago by Dr. Benson in the article "Bone" in "Todd's Cyclopædia," these veins pass through osseous canals, to the sides of which they closely adhere, so that they cannot close on their contents. Nor can they readily become occluded by lymph, which we know is Nature's expedient for preventing the dispersion of emboli or pus-clots. Their ready power of absorption was demonstrated by Cruveilhier's well-known experiment; he injected mercury into the medullary cavity of the tibia of a dog, and found it almost immediately after deposited in the liver. The fact that pressure on the femoral artery did not stop the pulsation convinced us of the inutility of pressure in this case, although so successful in aneurism affecting the larger trunks. Still less were we willing that the patient should lose his limb, although amputation has been the result of nearly every reported case.

We resolved to try the effect of actual cautery, and having chloroformized the patient, we pressed an iron button of the diameter of a shilling, heated to a white heat, deep into the tumour; a good deal of hemorrhage followed, but was repressed by muriated tincture of iron. In seven days the slough separated in small gritty pieces, and there was disclosed a mass of hard pulsating substance of the shape of large granulations, but of a pale colour. Seeing that it was necessary to re-apply the cautery, we did it this time with a sharp conical iron, which was thrust five times into the tumour, and thus it burnt its way for an inch and a-half from the surface of the tibia. In ten days a thick and somewhat conical slough came away, leaving a cavity filled with small healthy granulations at its sides, but with a small spot of rough bone at the bottom. This gradually became covered in, and the ulcer assumed the healthiest character. The patient has not suffered in any perceptible degree from the confinement to which he was necessarily subjected. I have warned him against any sudden or violent exertion of a nature which would tend to injure the arterial system, for there is no doubt that in some individuals a proneness to aneurismal disease exists almost in the manner of a diathesis.

Dr. Mapother added that the first operation was performed two months ago, and the second ten days after that. The tumour was now quite removed, and the ulcers were healed completely.

There was pain during the operation, but none without it.—*Dublin Medical Press, Feb.* 4, 1863, *p.* 106.

50.—ON THE TREATMENT OF NÆVUS BY TARTAR EMETIC.

By Professor Zeissl.

Professor Zeissl has found that nævi of medium size are best treated by the application of tartar emetic, which is both safe and effectual. It is true that this substance has long since been used for this purpose, and without much success; but in the employment of cauteries it is not of so much consequence which caustics we use, as how we use them. Neither a solution of tartar emetic nor the ointment will produce the desired effect. A plaster should be made of from sixteen to eighteen grains of tartar emetic, and one drachm of diachylon, and a considerable portion of this should be spread all over and somewhat beyond the nævus by means of the back of a strong knife, and kept *in situ* by strips of gummed paper. On the fifth or sixth day the entire surface of the nævus begins to suppurate, a crust gradually forming, which falls off in about fourteen days, leaving a most surprisingly slight cicatrix. If the suppuration is very profuse we may replace the plaster by simple oil dressing; but when it is not profuse the plaster may be left on until it falls off. When the plaster becomes accidentally removed wholly or in part, it must be renewed. Dr. Zeissl has repeatedly employed this application, both in children and in adults, without pain being produced. He has, however, never as yet resorted to it for nævi of the mucous membrane of the lips.—*Med. Times and Gazette, October* 25, 1862, *p.* 445.

51.—ON TREATMENT OF NÆVUS OF THE EYELID, BY PUNCTURE WITH A HOT NEEDLE.

By J. C. Wordsworth, Esq., Surgeon to the Royal London Ophthalmic Hospital.

[The patient was a child of only eight months old. The nævus affected the whole of the upper eyelid to such an extent that its motions were much impaired, and was rapidly increasing in size.]

The skin of the lid was of a dark-red colour, and completely involved in the nævus. On everting the lid, the mucous lining, too, was found to be marked by large vessels, being evidently part of the nævus that penetrated through the tarsal cartilage. When the child cried, a considerable swelling became apparent. I considered the case well adapted for treatment by the hot needle, and ventured to promise a complete recovery. The friends at once assented to my advice, and it was therefore submitted to operation the same day. A few inspirations of chloroform produced full anæsthesia—a condition most desirable for such an operation, for many reasons. A needle, on which is a bulb about one-third of an inch from its point, intended as a reservoir of heat, and set in a handle, was heated to *dull redness* in the flame of a spirit-lamp. The eyelid was raised from the globe by means of an ivory spoon, inserted into the palpebral sinus, so as to protect the eye, as well as to form a firm basis for the lid. A considerable number of punctures was then made through the entire substance of the lid, the needle being heated as often as necessary during the operation. In this way all the large vessels of the lid were apparently closed by the heat of the needle, punctures being made over its entire extent, at a distance of about one line apart. Wet lint was applied, and the case treated as a burn. Adhesive inflammation ensued, producing induration and subsequent contraction of the substance of the lid.

In about a month from the operation the nævus was reduced to a few scattered vessels over the lid which had escaped the influence of the cautery; and as they produced some deformity, and might probably increase in size and number if left, I repeated a few punctures around them. From that time the case has required no further treatment. The nævus has completely disappeared, leaving scarcely a trace of deformity. On the 7th of February the child was discharged cured.

The gratifying result of this case needs no comment. I believe that it could not have been equalled by any other treatment; indeed, from having adopted this means in several other cases, I am convinced that it affords such excellent results, and is at the same time so safe and so manageable, that it should supersede all others in the treatment of superficial nævi. I know of no other mode by which these growths, in such positions as this occupied, could be so effectually destroyed—without producing injury or deformity on the structures concerned. And as the only opprobrium to the actual cautery is now so effectually removed by chloroform, it is probably the most painless of all procedures that we adopt for the cure of nævus, and is divested of its terrors.

I may take this opportunity of adding one or two practical hints on the application of the cautery, as I have known disappointment occur through inattention to the principles involved in this mode of treating nævus; and I have myself failed to attain a cure by one operation where a little bolder use of the remedy might have sufficed.

1. As to the *temperature* of the needle. On this depends its influence, the object to be attained being the production of such an inflammation as will seal the vessels without causing death of tissue; a black or dull-red heat is required; in fact, such a temperature as will prevent the punctures from bleeding seems the most effectual. If the needle be applied (as I have witnessed) white hot, it simply burns a hole, and is productive of an eschar, instead of an induration.

2. As to the extent of the application, and the distance between the punctures. All visible vessels should be closed by

the cautery, and the skin rendered pale. To do this, punctures, about a line apart, should be made over the whole nævus ; and disappointment may be prevented by extending them a little beyond its boundaries to the normal tissues. This is better than having to repeat the operation, and does not add either to present suffering or produce subsequent evil.

3. In reference to a repetition of the cautery. Here, again, we must remember that contraction will continue for some time after the infliction of the burn, as under other circumstances of burning. Then, if we are too ready to repeat the cautery, we may not only be doing a work of superorogation, but may also produce greater effect than we desire.

For these reasons, having once effectually used the cautery, I am content to wait so long as there is any prospect of continued contraction before repeating it, unless I find the tumour still remains erectile, or some large vessels still patent and visible. With these precautions, I believe the profession will find this not only the safest but the most effectual of all remedies for the treatment of nævus. And I am fain to hope, that it will be yet more extensively employed to remedy those harmless but unsightly "mother's marks" that so often mar the beauty of many a fair face, and rob their unwilling possessor of the bright side of life.—*Lancet, March* 14, 1863, *p.* 294.

ALIMENTARY CANAL.

52.—PLUGGING THE NOSE.

By Dr. Cahill. (Abstract of a paper read before the Western Medical and Surgical Society.)

In cases of excessive epistaxis, the author had found that the ordinary mode of plugging the nose effectually, both in front and behind, was attended with considerable distress to the patient, and with some inconvenience to the operator, although it might not be attended with much difficulty. The method he had adopted consisted of passing a piece of dry and compressed sponge, about two inches long, and of sufficient size to pass easily along the floor of the nares. This was secured in the middle by a piece of tape or twine doubled. When the sponge was fairly pushed by a probe, or, what was better, a female catheter, through the posterior nares, the tape was drawn upon ; the sponge then took a direction transverse to the nasal cavity, doubling upon itself, and so blocked up, when swelled by the absorption of blood, the posterior nares. The two ends of the tape now passing out of the nostril being then separated, the anterior cavity was filled with cotton-wool, and the tape then tied across the whole in front of the nose. Removing it was easily managed by pushing the sponge into the pharynx, and so brought out through the mouth. The simplicity of this operation was shown by the author upon the skeleton.—*Med. Times and Gazette, Jan.* 3, 1863, *p.* 19.

53.—ON STRANGULATED FEMORAL HERNIA.

By Prescott G. Hewett, Esq., Surgeon to St. George's Hospital.

[After relating a case of strangulated femoral hernia, Mr. Hewett passes on to say a few words on different points connected with strangulated femoral hernia in general.]

First, as to the administration of chloroform in such cases. By many surgeons who have no hesitation in resorting to the use of chloroform in various operations, this anæsthetic is avoided in strangulated hernia, on account of the sickness and depression which occasionally follow its administration. Such symptoms after an operation for hernia would add greatly to the gravity of the case, and might, if severe and prolonged, lead to a fatal issue. Indeed, independent of operations for hernia, several cases are already known in which chloroform-sickness has gone on notwithstanding all remedies, and induced such a state of collapse that the patient has been in great jeopardy. And if this sickness should, perchance, occur after an operation for hernia, doubtless the danger of a fatal termination would be all the greater. It must be borne in mind, however, that chloroform-sickness of a severe character is, after all, of very rare occurrence; and, as far as my own experience extends, I cannot say that I have ever seen a case in which severe sickness has followed the administration of chloroform in an operation for hernia. Balancing, then, this evil of rare occurrence against the immense use of which chloroform has proved itself to be in the reduction of a strangulated hernia, I have no hesitation in resorting to its administration in such cases.

As to the taxis, that you know was not even attempted in the case which we have just been considering. And why not? The globular and pedunculated shape of the tumour made me think that this was one of the cases of strangulated femoral hernia in which the taxis would fail; and added to this there was the exquisite tenderness of the tumour, which showed that great harm might be done even by gentle pressure. In strangulated femoral hernia, the surgeon cannot be too careful in his attempts at reduction by the taxis; and, do what he will, the taxis will fail in by far the greater number of such cases—in three out of four, at the very least. Let it then be a rule with you in all cases of strangulated femoral hernia, and mind I am now speaking of femoral hernia only, to be very careful, very gentle, whenever you may use the taxis. If you are to succeed, depend upon it you will do so by gentle pressure, which ought not, moreover, to be prolonged beyond a few minutes ; if you fail, get the patient under the influence of chloroform, and then try the taxis again, but on no account with anything like force; and if you still fail under such circumstances, make up your mind to operate, and operate at once. The chances of success after an operation for strangulated femoral hernia may, literally, be calculated by the more or less time during which strangulation has been allowed to exist, and by the more or less force used in the taxis. And yet, what is the history of most of the cases of strangulated femoral hernia with which we have to deal in hospital practice? Rarely is it that the period of strangulation can be counted by hours: it is by days that it must be counted in by far the greater number of cases. Take, for instance, the twenty-two cases of strangulated femoral hernia in which I have had to operate in this hospital; in fourteen out of these twenty-two cases, the strangulation had gone on for two days and upwards, existing in seven out of these fourteen cases for three days; in two, for four days; and in one for five days. In one single instance, it had existed for five, and in another for seven hours, and in the remaining six, the least period was twelve hours. And whilst thus strangulated, the bowel had, with very rare exceptions, been subjected to the taxis, repeated at various intervals, and applied with more or less force. Indeed, in several of the cases, the force had been so great that the skin covering the tumour was of a dark purple colour, the cellular tissue extensively infiltrated with blood, the sac itself filled with bloody serum, and the bowel discoloured from blood extravasated in its coats.

The damage done to the bowel during its period of strangulation varies very much according to the size of the ring, and the consequent amount of pressure. Thus, in cases of recent hernia, cases in which the bowel is strangulated when it first comes down, the ring is very small and the pressure very severe; in such cases, a very few hours will often lead to irreparable mischief, and hence the reason why operations are, as a rule, so much more fatal in recent than in old femoral herniæ. The amount of pressure upon the bowel caused by the stricture is, then, the great source of danger in all cases, but especially in recent cases of strangulated femoral hernia; and hence the reason why I have so strongly urged you, if the taxis fail, to operate at once.

Opening or not opening the sac, about which there has been so much discussion of late, is, I am firmly convinced, quite a matter of secondary importance, compared with the period of strangulation, and the more or less forcible taxis.

For my own part, I invariably open the sac in operating for femoral hernia. I have never seen any harm arise from opening the sac; and great harm may arise, in some cases, from not opening the sac. For instance, in cases, and they are not uncommon ones, where the sac contains both bowel and omentum, and in which the omentum is more or less adherent to the sac, how can the surgeon be sure that a bit of the bowel is not left in the sac? Again, in cases where the omentum forms a complete sac enveloping the bowel, the stricture may be in the neck of the omental sac. Thus it happened in a case operated upon by Mr. Cæsar Hawkins. The sac, which was very thin, was laid open, and in it was found a large mass of omentum somewhat altered in structure. The neck of the sac and Gimbernat's ligament were divided, so that the finger could be easily introduced into the belly. The omentum was then carefully examined, and a knuckle of small intestine, of a very dark colour, was seen through its folds, which formed a complete sac; this omental sac was laid open; but, before the intestine could be returned, it was found necessary to divide the neck of this second sac, which formed a very tight stricture; the intestine was then easily reduced. And again, in recent hernia, where the thin, transparent sac has not as yet contracted adhesions with the neighbouring parts, and especially where it has become glued by recent adhesions to the bowel, the sac, if not opened, may be readily pushed up into the belly along with the bowel, and the strangulation thus continue. The possibility of such an occurrence was made manifest in one of my last operations, in which such a state of things existed; and had it so happened that I had not, in this case, opened the sac, it would have been pushed up into the belly when the bowel was reduced; and, as recent adhesions partially glued the bowel to the sac, and especially to its neck, the strangulation would have gone on.—*British Medical Journal, Feb.* 7, 1863, *p.* 131.

54.—REDUCTION OF STRANGULATED HERNIA.

By a writer in the LANCET.

After having recourse to the taxis, and every other means of reducing hernia, and failing, I procure a board, some five or six feet long and about fifteen inches wide, on which, when well padded with a folded blanket, pillows, &c., I place my patient, flex his knees over one end of the board, making fast (by means of a strap or handkerchief) the leg on the opposite side to which the rupture is situated. I then raise this end of the board gradually, to some considerable height, this position being maintained for some few minutes. In several instances I have found the tumour spontaneously disappear, from the force caused by the weight or retraction of the intestines; should this not occur, I have then recourse to further manipulation; and, as stated before, very, very seldom fail. When I have met with great difficulty, I have caused the board to be raised almost to the perpendicular, and really without much inconvenience to the patient.—*Lancet, Jan.* 10, 1863, *p.* 49.

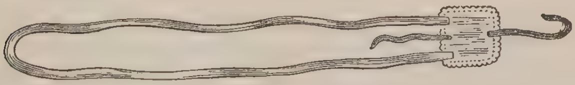
55.—ON A SIMPLE SUPPORT FOR CASES OF CONGENITAL RUPTURE AT THE GROIN.

By Charles Vines, Esq., Reading.

One of the principal difficulties we have to contend against in the treatment of the ruptures of young children, is the want of tact and perseverance, on the part of the mother or nurse, in returning the bowel whenever it has become displaced, and in keeping the bandage or truss properly adjusted.

With regard to the kind of appliance, I have seldom found steel, wire, or any kind of metallic truss suitable to young children. Trusses of this description are apt to become soiled and stiffened by the discharges, and the young infant can seldom bear the pressure and friction of the instrument. For some years I was in the habit of recommending a skein of wool, one end of which was looped round the thigh, and the other passed round the body, and fastened in the groin of the affected side. I cannot now call to mind the name of the gentleman who suggested this plan, which, in some cases, proved serviceable; but in the majority of the patients who came under my care, some more substantial support than that afforded by the wool was found necessary.

A lady, whose infant suffered from congenital rupture at the groin, and to whom I recommended the skein of wool, contrived a very simple yet efficient bandage to be worn in such cases. It consists of a long narrow loop of the finest flannel, rolled upon itself, with a pad of the same material at one end; attached to the pad are two little tape strings. The whole is represented by the following sketch:—



When worn, the circular end of the loop is passed over the thigh of the affected side, and the opposite, or padded end, is carried round the child's loins and opposite hip, so as to encircle the body. The pad is then fitted upon the seat of rupture, and secured underneath the loop around the thigh by the little tape strings. The mother should be provided with three or four of these little supports, so that every day, if necessary, a clean one can be used. The softness of the material, as well as a moderate degree of elasticity, constitute the advantages of this bandage. At least 40 cases of congenital rupture, principally occurring in my own practice, have been cured by the use of this ingenious, though simple, contrivance.—*Lancet, Dec.* 27, 1862, *p.* 699.

56.—CASE OF PROLAPSUS ANI OF FORTY YEARS' DURATION SUCCESSFULLY TREATED BY NITRIC ACID.

Under the care of C. J. Holthouse, Esq., at the Westminster Hospital.

J. C., aged sixty-three, an unhealthy-looking man, and the subject of chronic bronchitis, was admitted on the 29th of July for a bad prolapsus, which had existed for forty years. The whole circumference of the bowel protruded, and was red and vascular, the protrusion being apparently confined to the swollen mucous membrane, and not involving the muscular coat. The sphincter ani was relaxed, and exercised no constriction on the protrusion, the orifice of the anus being permanently open and occupied by the protrusion, which extended to a considerable distance beyond it. There were no hæmorrhoids; but the skin around the verge of the anus was much relaxed, superabundant, and thrown into numerous large folds. He was ordered confection of senna every night.

Aug. 5th. Strong nitric acid was applied to one half of the tumour without causing much pain, although no chloroform was administered.

16th. No unfavourable symptoms followed the application of the acid, and the protrusion is now less than half its former size. The acid was therefore applied to the remainder of the swelling, with as little suffering as before.

26th. The prolapse has retired within the sphincter, but the latter is still relaxed, and the anus dilated and fringed with several large folds of integument, some of which were removed with the scissors.

27th. The patient is up and walking about.

Sept. 19th. The patient has been about daily since the last note was made, and there appears to be no tendency to a return of the prolapse; but the folds of skin around the anus being still redundant, were now entirely removed with the scissors.

16th. Discharged cured.—*Lancet, Nov.* 15, 1862, *p.* 533.

57.—TREATMENT OF SOME FORMS OF HÆMORRHOIDAL TUMOURS AND PROLAPSUS OF THE RECTUM, WITH THE DESCRIPTION OF AN IMPROVED CLAMP.

By Henry Smith, Esq.

[The following is an abstract of a paper read by Mr. Henry Smith before the Medical Society of London, and of the discussion which followed.]

He commenced his paper with some observations on the great improvements which had been made in connexion with the treatment of these diseases of late, and some might fancy that we ought to be content with the very satisfactory results which are afforded by the use of the ligature and of the nitric acid; but he (Mr. Smith) had found so many patients object to the use of the ligature from a dread of dangerous consequences, that it was most desirable to substitute some method which would be equally successful as regards curing these diseases, and yet be free from the danger attendant upon the ligature. He had made use of nitric acid alone, and had been very much pleased with this agent; but he found that severe cases would not be thoroughly cured by it, and that in many, where such great benefit had accrued as to constitute a cure, sooner or later symptoms of the disorder would recur. He had therefore resorted of late to a method originally practised by the Dublin surgeons, and adopted by Mr. Henry Lee—namely, that consisting of a combination of excising and cauterizing. He (Mr. Smith) had, however, in the course of his practice, found the clamps ordinarily used to be somewhat awkward and inefficient, and he had had one recently constructed for him by Mr. Matthews, the blades of which were so shaped that they could readily be applied to the anal orifice, and would meet together closely by means of a groove. Moreover, through the handles of the instrument, which were made very thick and strong, was inserted a light but powerful screw, by means of which the compression could not only be put on the part to be removed, but could be as gradually taken off, and the instrument could be fixed at any period of the operation. This was a great desideratum in the process, for by this mechanism the part could be gradually and thoroughly strangulated, and the chance of bleeding could be entirely obviated. The author showed the instrument, and minutely described the proceeding which he adopted, and which consisted in first seizing the part to be removed by means of a vulsellum, then engaging the tumour in the blades of the clamp, which were next thoroughly brought together by means of the screw, and then fixed. After a few moments' delay the free portion of the tumour or prolapsus was cut off by sharp scissors. The cut surface was then carefully wiped, and strong nitric acid or actual cautery applied. The blades of the clamp were then gradually disengaged by means of turning the screw, and in this way any bleeding vessel was readily noticed and secured, and further cauterization could be made or a ligature placed on it if needful. The advantages of this plan of treatment consisted in its being free from danger if precaution be taken to prevent bleeding, and in its requiring much less loss of time on the part of the patient. In most cases only two or three days elapsed before the patient left his bed, and he was generally able to be about before the termination of a week. The author did not wish to decry the ligature; on the contrary, he had every reason to be highly satisfied with it, and was constantly using it; and his object was, not to substitute the method he had been recommending for the ligature in all cases, but to employ it in those cases where the patients objected to the ligature, or where there was some contra-indication to its use in the form of some organic disease, and in cases where the application of nitric acid alone would not be attended with successful results. The author detailed a few cases as illustrative of the proceeding. In one of these a great deal of bleeding occurred after the operation, in consequence of his attempting to secure two large tumours with one clamp; a portion of the divided membrane, in which was a large vessel, escaped from the extremity of the clamp, and could not be secured. This case he read as a warning not to attempt to secure two tumours with one clamp, but to operate on each separately, so that every portion could be thoroughly compressed and cauterized. The author concluded his paper with the details of a case where profuse hemorrhage, imperiling the patient's life, continued for some weeks after the use of the ligature, and was stopped ultimately by applying nitric acid.

Dr. Brunton inquired if the author had used the écraseur, which he had seen done many times. A great deal of stress was laid on the bleeding. Now the écraseur has been perfectly successful in arresting hemorrhage. In those instances where it did occur, the instrument was screwed up too rapidly.

Mr. Henry Lee thought the subject of the author's paper as important, so far as the public was concerned, as any in the whole range of surgery. He said that Mr. Smith's instrument was a great improvement, from the clamps being fixed. He (Mr. Lee) had used curved scissors with blunt ends, which prevented the tumours from slipping out of their grasp, which he described as of great importance for the application of nitric acid. The author, he said, laid great stress upon getting the bowel down. Now he had always advised its being allowed to remain up; it is this dragging of the bowel down that gives all the pain, and this was the objection to the use of the ligature, and also the great disadvantage of the écraseur. He had contrived a speculum some years ago for inward piles, with a sort of revolving canula, by which nitric acid or the actual cautery could be applied without pulling the bowel down. In some cases the bowel is so relaxed that the piles readily came down, and he referred to an instance that had been known to most of the profession for some years, in which the tumour was so large that the patient was advised not to undergo the use of the ligature; nevertheless he submitted to its application by Mr. Lee, and in a couple of months he was cured without any inconvenience.

Mr. Walter Coulson was disappointed that the author had not described the manner in which he employed the ligature. At St. Mark's Hospital they commonly used it, incising the base of the pile to receive it; and he had heard nothing that would lead him to believe that nitric acid was safer than the ligature.

Mr. Ashton understood the author to say that he had brought the subject of his paper forward, not because nitric acid possessed any advantages over the ligature, but because such fears were entertained of the effects of the latter. In 1843, Mr. Houston, of Dublin, made use of nitric acid in the treatment of certain cases wherein the bowel was not much affected. It had been asserted that the ligature was dangerous. Sir Benj. Brodie has recorded three fatal cases; they did not die from the treatment of their piles, but from disease of their kidneys or other organ. These have been cited as militating against the ligature, when in reality it had nothing to do with the result. He (Mr. Ashton) had not had a single instance in which there had been any inconvenience following the ligature. He referred in particular to one instance of a very large mass so treated with the best results. The author's plan he thought a modification of the operation; it took more time, and did not facilitate the patient's recovery. He could not understand how the clamp could give less pain than the ligature; and it would appear to be more difficult to strangulate the mass by it than by the ligature, and it must be difficult to destroy the base occasionally. The whole of the disease should be removed, and no more. It is not often, Mr. Ashton remarked, that the integument is involved, and it is highly improper to remove any of it. Sir Astley Cooper and others had lost cases from hemorrhage. He could not see the advantage that nitric acid possessed over the ligature, except that some patients will not submit to the latter. Mr. Ashton handed to the President an instrument similar to the author's, and which had been in use ten years.

Mr. HIRD observed that the older writers tell us that we are to cut skin and tie mucous membrane. The author honestly stated that he did not propose nitric acid as a substitute for the ligature; but he was dealing with internal hæmorrhoids and prolapsus, and he proposed the plan of securing the tumour before applying nitric acid and of removing it. Mr. Houston did neither. The case in which nitric acid is used principally were the raspberry form of hæmorrhoid—a class of affection attended by a large amount of hemorrhage, freely blanching the patient. This is more effectually treated with nitric acid than any other form. The author's cases were those in which the piles came down naturally, and these he proposed to treat by the clamp; but when we had an internal hæmorrhoid, rarely prolapsed, then we had a reason for adopting Mr. Lee's plan in such a class of cases. He thought the author's clamps possessed many advantages, especially in prolapsed bowel. No one could doubt that fearful hemorrhage sometimes occurred, and frequently caused death; he (Mr. Hird) had seen this bleeding take place, and he thought it an advantage to use Mr. Smith's clamp in such cases.

Mr. WM. ADAMS had used the écraseur in a great number of cases with great success—so much so, indeed, that he would not change this practice for any other. Occasionally there was a liability to secondary hemorrhage from nitric acid and the clamp. This was not so with the écraseur; it did happen to him once at the Great Northern Hospital, which was because the instrument acted badly. The great point, he thought, was to get time to perform the operation. He sometimes used two or even three écraseurs if there were several tumours, allowing ten minutes to get rid of each, going on to the notch alternately; this produced such a screwing up of the vessels that there was no hemorrhage. He generally transfixed them first by means of needles, and then applied the chain of the écraseur around them. He referred to the case of a gentleman with large profusely bleeding piles, forming a considerable tumour, which he thus treated, with the best results, the patient being cured in comparatively a few days. If an enema were administered beforehand, there would be no dragging or pain. He always used chloroform, and placed the patient on the side, the tumour being thus fairly in sight and readily removed.

Mr. ASHTON remarked that M. Nélaton had asserted that the use of the écraseur had been followed by traumatic stricture of the rectum in all cases that had come under his observation.

Mr. HULKE said if Mr. Ashton had not mentioned that fact, it was his intention to have done so, for an instance had come under his notice, and he knew it had occurred in Paris. He would remind the author that his treatment was liable to the same accidents as from the ligature, and he believed death had occurred even from the application of the actual cautery.

Mr. HENRY LEE remarked that if such cases had occurred, he would like the Society to know them.

Dr. EDMUNDS had found a strong solution of the chloride of zinc, in such cases as the author's, very satisfactory. If applied with a piece of wood or camel's-hair pencil smeared over the surface, it was followed by the best results.

Mr. MAUNDER said he was prejudiced, perhaps, in favour of the ligature. He could not understand why the écraseur should be injurious, unless too much mucous membrane were removed. He thought the author's paper went to do away with the ligature; the clamp being recommended to assist in cutting the tumour, and the nitric acid to arrest the bleeding.

Mr. HUNT observed that it appeared to him that one important point had not been brought forward,—and that was, that in all the modes of operation success followed, and in a certain proportion hemorrhage would occur. He inquired what was the cause of this hemorrhage, that took place in one out of fifty or a hundred cases. He himself thought it was due to incomplete removal of the morbid structure. In the author's method the operator had, probably, a greater command over the pile than in any other; and if the nitric acid were applied properly, it appeared to him that the patient was perfectly safe, if the acid were very pure and strong.

Mr. GAY had long had confidence in the use of the ligature, except to those vascular tumours of the mucous membrane to which the nitric acid was the most suitable. He thought the loose tumours of the rectum should not be confounded with prolapsus. In these cases he had found the ligature the most serviceable. He had never seen or heard of tetanus occurring, but he thought it more likely to arise from the use of the écraseur than from any other method.

The PRESIDENT asked what were the cases in which the author prefered his own plan to the ligature; what kind of cases were suitable; and how long the pain lasted.

Mr. SMITH, in his reply, stated that it was in those cases where the tumours were not very large nor very vascular, and were readily brought down, that he preferred the clamp to the ligature. With regard to the pain, he said it differed much according to the temper of the patient, and the manner in which the operation was done, and if none of the skin was included. Its duration was momentary—while the blades of the clamp were being screwed together. The application of nitric acid produced no pain at all, because sensation was destroyed by the clamp. Sometimes some of the skin was included, when the pain lasted perhaps a quarter or half an hour, and he had known it last some three or four hours. He had employed the ligature to a great extent, but he found a number of patients who had so great a dread of it that he looked out for something else; and the nitric acid seemed most serviceable with the clamp, in the manner he had already described. He had known of two cases of tetanus, one of which occurred two months ago in the practice of one of his friends, an hospital surgeon; and both died. Some years ago there were four in one season at St. Mark's Hospital. Tetanus was then prevalent. He believed that stricture resulted from some of the healthy structures being involved in the écraseur, and therefore strongly deprecated its use.—*Lancet, Nov.* 29, 1862, *p.* 592.

ORGANS OF URINE AND GENERATION.

58.—CERTAIN ERRORS IN THE DIAGNOSIS AND TREATMENT OF RETENTION OF URINE.

By Barnard Holt, Esq., Senior Surgeon to the Westminster Hospital.

Having lately had under my notice several cases in which serious errors have been committed, both in the diagnosis and treatment of cases of retention of urine *not dependent upon stricture of the urethra*, and in which that canal was in a healthy condition, I wish to bring their salient points under the notice of the profession, together with some remarks upon the treatment pursued.

Retention of urine in a healthy man may be said to occur generally from one of three causes:—1st. From over-distention consequent on the postponement of the act of micturition. 2nd. Upon spasm arising from irritation of the urine after a debauch. 3rdly. From either or both of the above acting upon an elderly person subject to more or less enlargement of the prostate.

One would imagine that in all cases of retention the history of the case and the urgency of the symptoms would sufficiently indicate that the patient is suffering from retention and not suppression of urine, even if the point had not been so strongly insisted on as it has by all modern writers upon the subject. The resulting dribbling away or overflow from a distended bladder has also been fully described; and I cannot do better than quote one of the latest descriptions (Cooper's "Surgical Dictionary," art. Bladder):—

"Under these circumstances (loss of power) the bladder becomes distended, and, when the distension is excessive, the sphincter action of the neck of the bladder is overcome, and the urine dribbles away by slow degrees in sufficient quantity to prevent *further* distension, but without relieving that which already exists. A practitioner unaware of this might mistake the case for one of incontinence of urine, in which there is inability to *retain*, instead of inability to *expel*, the urine."

After describing the symtomatic tumour in the hypogastric region, and the dulness yielded by percussion of a distended bladder, the author quoted says: "In cases where the surgeon is in any doubt, however, the introduction of the catheter will at once clear up the difficulty." It is here, however, in my experience, that a fallacy occurs which constantly leads to most serious error. The surgeon introduces a silver catheter into the urethra, it is true; but the instrument never enters the bladder, being either too short (as is frequently the case, especially when the prostate is enlarged), or, being thrust onwards, makes a false passage in which the point becomes entangled. Or, again, the surgeon employs an elastic catheter without a stilette, under the impression that thus no damage can possibly be done; and the instrument is then either too short, or, as is frequently the case, it bends upon itself, and never reaches the bladder at all. It has happened that a catheter has been blocked with coagulum, &c., which has not allowed the passage of urine even after the catheter has entered the bladder; but only great carelessness could overlook this.

The only way to be perfectly certain that a catheter has reached an *empty* bladder is to inject the water through it, which it will readily admit; whereas if the instrument has *not* reached the bladder, it will be impossible to inject more than a few drops. The same plan may be adopted in those rare cases where a *full* bladder has been reached, but the instrument has become temporarily obstructed with clot, which prevents the exit of urine.

The following two cases are good examples of over-distention of the bladder, resulting in loss of power, and consequent retention; one of which subsequently proved fatal, owing, no doubt, to unrelieved distension of the bladder, and the other, being mistaken for a case of suppression of urine, also terminated fatally.

Case 1.—In January, 1861, I was requested to visit Mr. M., a hale, hearty man, aged sixty-two, who was suffering from retention of urine arising from over distension of the bladder. He told me that whilst travelling from Birmingham to London he had, after leaving Rugby, an urgent desire to pass urine, which he was compelled to repress, and when he arrived in London he found it impossible to relieve himself of even the smallest quantity. He drove to his house as quickly as possible, and sent for his surgeon, who attempted to pass a catheter but failed. There was considerable hemorrhage from the urethra, but no urine passed; the patient was therefore placed in a warm bath, and twenty minims of tincture of opium were administered. At midnight I was requested to see him, when a superficial examination sufficed to show that the bladder was greatly distended; and upon passing a *prostatic* catheter (which was accomplished without the slightest difficulty), more than a quart of urine was removed.

Vivid recollections of previous cases in which grave errors had been committed, induced me to point out particularly to the practitioner in attendance the necessity of introducing the catheter at least four times in the twenty-four hours, in order that the bladder might be kept moderately empty, and so be enabled to recover its tone; but that gentleman, believing that all difficulty in the treatment of the case was over, did not think it necessary that I should see the patient again.

A fortnight afterwards I had occasion to meet the same practitioner, and upon inquiring about his patient, learned that he was "dead and buried!" I afterwards ascertained that the bladder had not been relieved by the catheter, as I had advised, because the urine had dribbled away after many hours' retention, and it was therefore believed that there was not any in the bladder.

Case 2.—In March, 1861, I was requested by another surgeon to see a case of supposed "suppression of urine." The patient had been in the habit (owing to his occupation) of retaining his urine for a long time; but on the present occasion he had neglected to relieve his bladder when the desire was urgent, and when at length an opportunity offered he found himself incapa- of evacuating even the smallest quantity. His sufferings, as in the previous case, were most severe, the desire to pass urine becoming more urgent every hour, and being accompanied by the most violent but futile strainings. His surgeon attempted to pass an *elastic* catheter, and as he believed successfully, since the whole length of the instrument was introduced; but no urine escaped. This was repeated with some difficulty on three or four occasions, and was followed by smart hemorrhage, but without in any way relieving his patient. Believing that the patient was suffering from suppression of urine, the surgeon ordered fomentations to the region of the bladder, and administered nitrate of potash and digitalis, and, as is usually the case, after several hours a small quantity of urine was voided, and it continued to dribble away for several days, the constitutional symptoms being severe, and gradually assuming a typhoid character. At this stage of the case I was requested to visit the patient, and found, as in the former instance, that the most superficial examination revealed the nature of the case, the bladder being enormously distended, and forming a prominent tumour above the pubes, which yielded a dull sound on percussion. I introduced a No. 9 silver catheter with care, and drew off *two quarts of urine*, and another quart was removed in the evening. Although every attention was paid to the after-treatment of this case, the patient gradually sank, and died within a week of my seeing him.

In the following cases, in addition to the urgent symptoms consequent upon an over-distended bladder, the patients lost a considerable quantity of blood from the bladder, which was not effectually arrested until I had recourse to the administration of turpentine in ten-minim doses, the good effects of which I have frequently witnessed in such cases.

Case 3.—Towards the close of last year I was requested by telegram to proceed to a town more than a hundred and fifty miles distant to visit a gentleman who had alarming vesical hemorrhage. Upon my arrival, I found the patient, who was sixty-two years of age, pallid and feeble, partly from loss of blood, and partly from the exhaustion incidental to low irritative fever. On inquiry, I found that my patient who up to ten days before had enjoyed most excellent health, had had, whilst engaged in transacting important business, an urgent desire to void urine, but was unable to quit the room for that purpose, and that when able to do so a short time afterwards he had found himself incapable of micturating. A surgeon was sent for, who attempted to introduce a catheter into the bladder, and the instrument was said to have *passed for its whole length*, but no urine was withdrawn. Similar attempts were repeated three or four times (as in Case 2.) but with the same result; and, although the patient was suffering the most intense agony, the case was now considered to be one of "*suppression*," and linseed-meal poultices were applied to the abdomen, whilst the patient was desired to drink plentifully of barley-water and linseed-tea, in order "*to force the water.*"

On the following days the urine was passed in small quantities at short intervals, the swelling and agony remaining unrelieved; and the patient became much exhausted and passed into a typhoid condition. After having been allowed to remain with a distended bladder *for six days*, another surgeon was consulted, who passed a catheter without difficulty, and *removed three pints of bloody urine.* This gave great relief, and the patient *rallied;* but the urine which was drawn off only twice a day, still contained a large quantity of blood, which was unchecked by gallic acid, tincture of iron, and turpentine in two-minim doses, and I was therefore summoned.

On my arrival, I at once introduced a gum-elastic catheter, and removed a pint of urine loaded with blood; and having then washed the bladder out with cold water, I fastened the catheter in, and directed the patient to evacuate the bladder every four hours. Turpentine, in ten-minim doses, in mucilage, was given internally, and repeated every four hours, and produced an almost immediate effect upon the hemorrhage. In consequence of the coagulum which remained in the bladder, the eye of the catheter became blocked during the night, and the patient was unable to relieve himself. I therefore removed the instrument, and introduced one with a larger eye, through which the urine, containing now but little blood, flowed freely. On the following day the urine was nearly natural, and the patient somewhat improved in general condition; and the reports subsequently forwarded to me state that the appetite has returned, the bowels are regular, and the urine is free from blood, but that the patient *has as yet no power of expulsion*, and is still obliged to have the catheter employed. Iron and quinine are now being administered, and the urine is withdrawn three times during the day, and twice in the night. The patient is able to take a daily airing in his carriage; but it remains to be seen whether he will recover the power over his bladder.

The cases I have already described may be termed *acute*, but there are others of a more *chronic* character in which mistakes of a similar kind are continually occurring. I mean those in which there is frequency of micturition of some months' duration, accompanied by great general irritation of the system and *excessive thirst*, and in which the fact that the patient never thoroughly empties his bladder, but leaves a constant *residuum* of urine, has been entirely overlooked. The following is a good case in point :—

Case 4.—Mr. F., residing on the south coast, consulted me last year in consequence of frequency of micturition, which necessitated his relieving himself every three-quarters of an hour, both night and day, His health was greatly impaired from want of rest, he was very irritable, and had pain over the pubes and *excessive thirst*. He had consulted several surgeons, who had prescribed various remedies without relief. A careful inquiry into the history of his case assured me that the bladder remained always more or less permanently full, although the ordinary quantity of urine was passed during the twenty-four hours, and although his urethra readily admitted, as he told me, a No. 10 catheter. He had no power of ejecting the urine *any* *distance from his body*, but it came away in a dribbling, uncertain stream which dropped down between the legs. I desired him to micturate before me, when only about an ounce escaped; but upon introducing a catheter I drew off five ounces of offensive urine, and upon injecting the bladder with warm water found that it would not hold more than six ounces. The nature of the case was thus obvious: for six months this gentleman had been suffering from retention of urine, although he passed as much during the twenty-four hours as the kidneys secreted. I desired him to pass his catheter every six hours, and to wash out the bladder every night, gradually increasing the quantity of water injected, so as, if possible, to distend the bladder eventually to its normal capacity. He was ordered to live well, and take wine (from which he had previously been debarred), with the tincture of steel twice a day.

In a few days he had so far improved as not to require to pass urine more frequently that every four hours, and he now has improved materially in general health; his *thirst* has entirely subsided, and he has gained flesh, whilst the capacity has so improved that he has to relieve himself only once during the night. It is more than probable that this gentleman's bladder may never recover its tone, and that the error of treatment committed will necessitate *the constant use of a catheter for the rest of his life;* but as he has gained some slight power of expulsion during the last few weeks, it is just possible that he may alleviate his unfortunate condition.

The foregoing cases, which I have selected from among a considerable number that have come under my notice, will sufficiently illustrate the errors in diagnosis and treatment which are but too common. The history of each was so distinct as to make it difficult to conceive how any error could have occurred. All the patients had been in good health, and free from any urethral or kidney complications; and though in one there was slight enlargement of the prostate, in all the retention was due to paralysis of the bladder consequent on over-distension. In fact, the surgeons under whose care the cases first came were of that opinion, and attempted the introduction of catheters unsuccessfully, and then, putting the cases down as examples of "suppression," were afterwards misled by the dribbling or overflow, which they took to be the re-secretion of the kidneys stimulated by the measures they had adopted.

The diagnosis between retention and suppression is so very easy as to render a mistake perfectly inexcusable. In retention there is the urgent desire to micturate, accompanied with violent spasms, not only of the urethra and perineum, but of the whole abdominal wall; and as time elapses, the *urgency in-*

creases, the patient rolling in agony, and straining violently to relieve himself. Besides, the surgeon's hand will at once detect the solid tumour above the pubes, formed by the distended bladder, which will yield a dull sound on percussion. In suppression of urine, on the contrary, there is no urgent desire to micturate, no spasm, and no agony consequent on a distended bladder; but the patient lies in a listless condition, soon passing into coma, whilst the breath and skin exhale a strong urinous smell. Moreover, the bladder will be found empty, and the fingers can be thrust into the pelvis, where the intestines yield a clear percussion sound. It must not be forgotten that a case of retention will at length pass into a typhoid condition, which might possibly be mistaken for the coma of uræmic poisoning; but the history of the case, and the presence of a distended bladder and dribbling of urine would at once point out its true nature.

In all the cases I have seen, the error arises from the catheters not having entered the bladder. Surgeons in general practice, who are not much in the habit of passing catheters, usually introduce a gum-elastic catheter without a stilette, which, if it meets with even slight resistance, is very likely to bend upon itself, and thus never reach the bladder, although its whole length may have been introduced into the urethra. As I remarked in the early part of this paper, the injection of warm water at once clears up any doubt, and the fact that water cannot be injected may be considered conclusive evidence that the catheter has *not* reached the bladder.

I have no hesitation in saying that in all cases such as I have described a catheter *can be passed* into the bladder, and I conceive it to be unjustifiable in any surgeon to be satisfied until he has withdrawn the urine; in which, if he will employ a metallic instrument of moderate size, he will in all probability succeed with ease. Time is of the greatest moment in these cases; and if, therefore, the surgeon in attendance do not succeed in his attempts, he is bound to call in assistance without delay, or his patient may possibly lose his life, or at least be condemned to the misery of the use of the catheter for the rest of his days.

The more I employ it, the more I feel satisfied with the use of turpentine, in ten or fifteen-minim doses, in the cases complicated by hemorrhage from the bladder. In Case 3 it acted at once, although both gallic acid and the muriated tincture of iron had been employed without benefit; and I think the drug deserves a more general recognition by the profession. —*Lancet, Feb.* 21, 1863, *p.* 202.

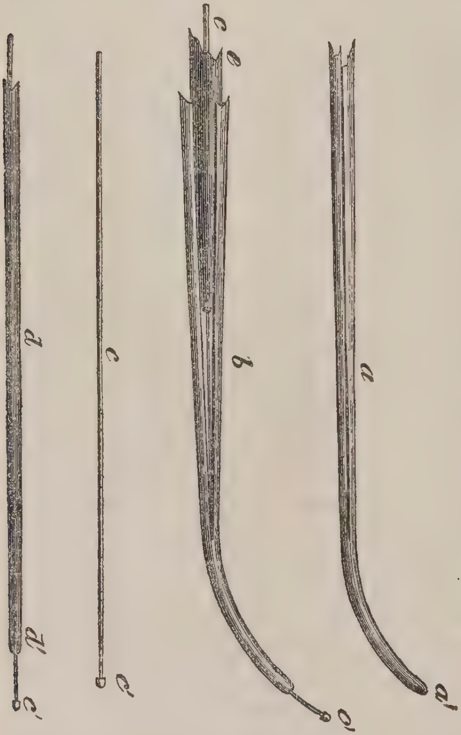
59.—ON THE TREATMENT OF STRICTURE OF THE URETHRA BY THE MORE "IMMEDIATE PLAN."

By Dr. Philip Crampton Smyly, M.A., one of the Surgeons to the Meath Hospital.

In the November number of the Dublin Quarterly Journal, page 297, there is a very able essay on Mr. Bernard Holt's "Immediate Plan," by Prof. Macnamara. I refer to this paper, and also to Mr. Holt's own very interesting book, in order to call the reader's attention to the fact, that in almost every case a certain amount of dilatation was necessary, before the dilator could be introduced. This is indeed self-evident, as the dilator itself is equivalent to No. 3, and many instruments I have seen are so large as No. 4 catheter. In the case of T. N., p. 303, in Mr. Macnamara's paper, I passed No. ½ for several days before I could get in No. 1, and several more before I could increase the size to No. 3. Even when dilated so far there was much difficulty in introducing the metallic dilator. After the dilator was once in the bladder, nothing could be more satisfactory than the conclusion of the operation, and the whole course of the case. I have since, on more than one occasion, passed No. 11 into this man's bladder without the slightest difficulty. Mr. Holt does not mention, in all his cases, whether he employed this preliminary dilatation or not. In case No. 1 he evidently did not. He says (page 13):—"After the lapse of a third week, and while the patient was fully under the influence of opium, a third attempt was made, but with no better success. I therefore determined to place the patient thoroughly under the influence of chloroform, when, after very considerable difficulty, a No. 1 catheter was introduced; great constitutional disturbance followed, so that a month elapsed before any further instrumental attempt was made. Former experience having shown the improbability of introducing any instrument, excepting while under chloroform, he was once more anesthesiated, and the dilator, with difficulty, passed into the bladder." In Mr. B. Holt's hands such a proceeding may be without much danger. But we all know that there are such things as false passages, and also that a very good surgeon *may* make one. And though, as Mr. Holt says, "the operation is of the most simple kind, and any one who can pass a bougie through a *difficult* stricture is competent to perform it," I would ask the young surgeon—or even a somewhat experienced surgeon—if he would not have some hesitation in bursting a stricture with No. 10 or 12 tube, through which he had forced a metallic instrument, equal in size to No. 3 or 4 silver catheter, the stricture being so tight as only to admit No. 1?

I do not make these remarks in any way to disparage Mr.

Holt's plan. My object is simply to call attention to an instrument I have devised, which combines the advantages of Mr. Holt's plan and of the method employed by Mr. Hutton of this city, which is familiarly called the "railway catheter," and has been still further developed, recently, by Mr. Wakley.



(*a*) The dilator closed.

(*b*) The branches slightly opened to show the wire guide in the position of the stilette in Holt's instrument. The dilator (*e*) is not quite pushed to the end.

(*c*) The guide of silver wire.

(*d*) No. 1 catheter, either of silver or gum elastic, to which any curvature may be given.

The instrument consists of a No. 1 catheter (*d*), or even smaller, having an orifice at the point. The guide (*c*) is about twice the length of the catheter, and is terminated at the vesical end by a small silver button (*c*'), which exactly fits the orifice of the catheter (*d*'). There is no ring at the opposite end of the guide, so that the catheter may be withdrawn from the bladder without removing the guide.

The dilator (*a*) is, with a few exceptions, similar to Mr.

Holt's. The differences are :—1st. There is no stilette; 2nd. The point is perforated (a') to fit the guide I have described above; and 3rd. The point of the instrument is made conical. The shafts are grooved exactly like Holt's, and the arrangements about the handle are identical.

The mode of using this instrument is sufficiently simple. The catheter, armed with the guide, is passed through the stricture into the bladder. The catheter is then withdrawn over the guide which remains in the urethra; the catheter (d) is partially withdrawn to show the guide. The dilator is substituted for the catheter, as Wakley would substitute a catheter of a higher number. It is pushed down through the stricture until stopped by the button (c') at the end of the guide in the bladder. The instrument is now exactly Mr. Holt's, and the further steps of the operation, namely, the bursting the stricture, may be completed by passing down any of Mr. Holt's tubes on the guide. I should further remark that the catheter may be either of gum-elastic or of silver.

The advantages of this combination appear to me to be the following:—

1st. The stricture may be dilated at the first passing of an instrument.

2nd. That, with patience and gentleness, a stricture may be passed with No. 1, in very inexperienced hands, where even the most talented surgeon, even with chloroform, would fail to introduce No. 3.

3rd. That a gum-elastic catheter, with a fine stilette, may often be passed into the bladder when it is very difficult or impossible to pass a metallic instrument of the same number.

4th. That, whether the instrument first introduced be metallic or elastic, a certain guide is left behind for the dilator, which can then and there only be forced through a tight stricture without fear.

And 5th. I may add that as the instrument does not require to be so strong as Mr. Holt's, it can be made much smaller in size.—*Dublin Quarterly Journal, Feb.* 1863, *p.* 80.

60.—ON THE SUCCESSFUL TREATMENT OF SEVERE STRICTURE OF THE URETHRA BY GRADUAL DISTENSION AT A SINGLE SITTING.

By Henry Thompson, Esq.

(Being an abstract of a paper read before the Royal Medical and Chirurgical Society.)

[By severe and obstinate strictures Mr. Thompson intends to denote those which are little benefited by dilatation. The proceeding advocated is called "gradual distension," and is different from dilatation simple and continuous; from rupture or "instantaneous treatment;" from cauterization; and from incisions.]

By the process in question the strictured part of the urethra only is acted upon, and this not to a degree short of, but up to or even beyond the natural calibre of the canal, wherever the stricture may be situated. All this is accomplished at one sitting, but with gentleness and slowness, so as to avoid unnecessary rupture; the degree of distenson being regulated with absolute certainty, and its extent indicated with extreme accuracy, by apparatus in the handle of the instrument employed.

The object of the operator is not to rupture, but to over-distend the fibrous tissues which constitutes the stricture, so as to destroy, or at all events to impair, its natural tendency to contract. He aims at attaining that result which occurs from the practice of over-distending vital tissues elsewhere—viz., to impair or destroy their contractility. This is known to happen after the application of over-distension to both healthy and morbid tissues, and is turned to account by the surgeon for that purpose.

The class of patients for which this proceeding appears to be best adapted is described, and illustrative cases are appended.

The instrument consists of two long and narrow steel rods, accurately applied throughout their entire length by the single plane surface which each possesses. The external surface of each rod is convex, so that together they form a nearly cylindrical instrument, but tapering towards the lower extremity, where they are closely united. At the opposite or upper end they are also united, and are surmounted by a handle resembling that of an ordinary sound. This handle is attached to a screw with a very fine thread, which being turned causes the two rods to diverge very slowly and very gradually from each other at a given spot, about six inches from the handle. When the separation of the blades is effected, an index, placed near the upper end and connected with some numerals on a disc, shows the exact degree of extension made by pointing out that number of the catheter-scale to which the distension existing at that moment is equivalent. The general form and contour of the instrument is that of a slightly curved catheter. When the screw handle is turned, the two rods separate, so as to form a long oval or spindle-shaped figure, the long diameter being about three inches and a half or four inches in length, and the short diameter corresponding to the number of turns given to the screw, and varying between the slightest possible separation of the rods and an interval of about three-eighths of an inch, ro even more.

The stem of the instrument has marked on it a graduation in inches, which commences one quarter of an inch below the point of maximum distension or centre of the spindle-shaped figure produced by the separated rods. It is that point which will correspond with the stricture when the instrument is placed in the urethra, so that the contracted portion of the canal undergoes the greatest amount of distension which it can be desired to produce, while the rest remains wholly unaffected.

The mode of applying the instrument is as follows:—A medium or full-sized bougie or catheter is first passed as far as to the stricture, and the distance from it in inches to the external meatus carefully noted. Suppose it to be five inches, the operator, taking the distending instrument, places the little blue steel collar which slides on its shaft opposite to the figure 5, and passes the instrument through the stricture until the collar arrives at the meatus of the urethra, and prevents the instrument from entering further. The maximum point of distending power must, therefore, correspond with the narrowest part of the stricture. The act of distension is now commenced by making two or three turns of the screw-handle, and is continued by slowly turning it once every half minute, taking care at the same time to prevent the instrument from shifting its position by observing that the collar remains opposite the external meatus.

In a short time the index, gradually rising, shows that the calibre is reaching Nos. 10, 11, 12, and so on, until in a few minutes No. 14 or 16 has been reached, which latter limit is usually quite sufficient. The screw-handle is now slowly turned backwards, not the whole way, but until the index has retreated to about No. 8 or 9, when the instrument is withdrawn. The operator next passes a full-sized gum catheter into the bladder, and fastens it there, leaving it in place for about twenty-four hours. It is then removed altogether. All that remains to be done is to pass a full-sized metallic instrument every second day for a week, and after that at increasing intervals for a week or two longer.

Mr. Holt said that his mode of operating having been alluded to by Mr. Thompson, he was desirous of offering a few remarks upon the plan which that gentleman had proposed to the Society, as contrasted with that which he (Mr. Holt) had adopted for some years. Mr. Thompson's instrument differed materially from his, both in the method of its application and its capability of enlarging the strictured portion of the urethra, and he claimed for his instrument the power of dilating the stricture to No. 14 or 16, while the meatus is not interfered with. For its application, however, it was necessary a gum-elastic catheter should be retained in the urethra for two or three days. It was then removed, and the seat of the stricture accurately ascertained by passing a large bougie. This is to be marked, and such measurement is to regulate the distance which his dilator should be introduced. The patient being now placed under the influence of chloroform, the dilator was passed, and the handle slowly and gradually turned, so that dilatation could be effected to any extent, the whole operation occupying from ten to twenty minutes, according to the character of the obstruction. The dilator was now removed, and a No. 12 gum-elastic catheter substituted, and retained in the bladder for two or three days, according to circumstances. A No. 12 bougie was afterwards passed at intervals of a day, and so the after-treatment was continued as in any other plan. Now his (Mr. Holt's) first objection to this method of treatment applied to a difficulty which might arise in the hands of a surgeon less accustomed to the passing of bougies than the author of the paper. The penis during an examination alternates in its length, and is at one moment quiescent, and at another semi-erect. When this occurred, it would so alter the relative measurements as to render them of little value; and this part of the treatment was of great practical importance, inasmuch as it was necessary that the centre of the dilating power should exactly correspond to the centre of the stricture, and without it the dilatation would be either in front or behind the obstruction, and so be perfectly ineffective. His next objection would apply to instances in which there was more than one obstruction; for in his experience there were frequently two, and sometimes three or even four obstacles to be overcome, and in these instances it would be necessary to materially prolong the operation, or repeat it upon three or four separate occasions. Mr. Thompson laid particular stress upon his capability of dilating the structure to No. 14 or 16 or more, when the meatus will only admit a No. 12. In the first place, he (Mr. Holt) believed the author was in error in supposing that a dense obstruction would allow itself to be dilated to the size he mentioned; it must be ruptured, as was shown by the occurrence of hemorrhage. But even granting that it was so, he (Mr. Holt) could not see the least advantage that could arise from dilating a stricture beyond that which could be maintained by the after-passage of a bougie. The part which was so dilated quickly contracted, otherwise there would not be any necessity for the after-passing of a bougie; but as this was necessary, and the bougie could only represent the size of the meatus, no advantage could accrue from distending the stricture beyond its natural limit. If any further proof were required that this was so, he might refer to the median operation of lithotomy, which he had frequently performed, and removed, without cutting the neck of the bladder and prostatic portion of the urethra, calculi varying from an inch to an inch and a quarter, the parts being dilated by the introduction of the finger, the subsequent passage of the forceps, and the extraction of the stone. In all these cases the patient had retained the power of expelling the urine at will, which specially showed how speedily any undue dilatation of the urethra contracts again. His (Mr. Holt's) third objection would apply to the necessity of retaining a catheter in the bladder, both before and after the operation had been performed. In many instances the patients were quite incapable of bearing the retention of a catheter from the pain and irritative fever it produced, and it confined them to the house or bed for an unnecessary period of time. Mr. Holt then related a remarkable case of irritable and intractable stricture in which, after all the usual means employed for a long period had failed in the hands of an experienced surgeon, he had, in a few minutes, split up the stricture, with his instrument, and the patient recovered without a bad symptom, and remained well to this time—a period of twelve months. Were he not afraid of occupying too much of the Society's time, he could relate several most interesting cases to prove that he had not made any selection for the purpose of enhancing the success of his operation; but that, on the contrary, he had operated upon the most severe and complicated examples that could be submitted to the surgeon. Two other cases he would briefly allude to. One was that of a gentleman from Liverpool, who consulted him for incontinence of urine, from which he had been suffering for two years, during which he was compelled to wear a urinal, and his health was so damaged that he was recommended to go to Madeira for a change. Upon inquiry Mr. Holt was satisfied that the bladder was full, and, having gained the patient's consent to introduce a catheter, he with great difficulty passed a half No. 1, and removed three pints of most offensive purulent urine. His bladder was paralysed, and there was every probability, from the length of time it had been distended, that the ureters and the pelves of his kidneys were abnormally enlarged. For the purpose of keeping the bladder empty, the catheter was introduced three times during each of the first two days, and upon the third he passed the dilator and split the strictures, so that the urethra would admit a No. 10 catheter. In one month this gentleman returned to Liverpool, with his health almost entirely re-established, passing urine with the greatest facility, and the urethra admitting a No. 10 easily. During the last month, Mr. Cutler asked his (Mr. Holt's) opinion upon the case of a gentleman who had been operated upon by Mr. Syme. The patient had subsequently been subjected to internal division, and a second time Mr. Syme's operation was performed by Mr. Bickersteth, of Liverpool. When he consulted Mr. Cutler, he was obliged to pass the catheter five or six times in the twenty-four hours, and he was entirely prevented, by the escape of urine during the intermediate periods, from entering into society. Mr. Holt, having explained to him that in such a case he could not positively affirm what the result might be, consented to split the obstructions; but the patient, being exceedingly nervous from the recurrence of rigors after every attempt to enlarge the urethra, would not consent to the operation without the aid of chloroform. The dilator having been passed, it required all the force Mr. Holt could make use of, even with a towel placed over the large extremity of the tube, before the stricture could be split. Upon the dilator being removed, a No. 10 catheter was passed, and the bladder emptied. This gentleman never had a bad symptom of any kind or description; he never even went to bed, and in two days was able to visit some friends in the country. He declared that he could now pass urine better than he ever remembered; and in a fortnight he returned to Jersey, being capable of introducing an instrument himself without the least difficulty or pain. Mr. Holt stated that he had now operated in hospital and private practice, upon more than two hundred and fifty cases, without any complication of either infiltration of urine, abscess, swelled testes, or inconvenience of any kind, further than the occasional supervention of a rigor or mild attack of stricture fever. He must therefore still retain his opinion in favour of his instrument, which had now been tested to the utmost. It was perfectly simple, and capable of being used by any surgeon who was able to pass a catheter. The operation did not require either the previous or after retention of a catheter in the bladder; it was completed in one second, however many obstructions there might be; it did not, excepting in very severe cases, require the administration of chloroform, and the patients were not confined to the house longer than the afternoon in which the operation was performed; the pain was of the most trifling description, and the danger *nil*. He would only detain the Society another minute in describing the improvement he had lately made in the dilator, and which had only been completed that day. The objections raised to it by some surgeons were, that in its present state you had no positive evidence when you were in the bladder, and that the tube might possibly slip from between the blades. He had now so improved upon this that it was quite impossible the tube could so escape, and the dilator acting as a catheter permitted the flow of urine. He had no hesitation in declaring that, as now manufactured by Messrs. Whicker and Blaise, of St. James's-street, the dilator was as perfect as any instrument could possibly be.

Mr. Solly was pleased at any proceeding which showed the advantage of dilatation over cutting in the treatment of stricture. He contended that if an instrument could once be passed into the bladder, cutting was unnecessary. There might be cases in which it would be impossible to succeed in such an effort, and then incision might become requisite. He had never had recourse to the plans recommended by Mr. Thompson and Mr. Holt, but had been in the habit of adopting a modification of Mr. Wakley's plan, previously resorted to by Hutton, Dublin. He used a catgut bougie instead of the metallic guide, and elastic catheters instead of the silver ones. He had used this plan of gradually dilating the urethra with much success. In hospital patients he allowed the catheter to remain in the bladder for some time, but not more than half an hour in private practice.

Mr. Fergusson said it had been proved that dilatation of the urethra might be effected, even to the extent of a No. 12, 14, or 16 catheter, in a few minutes, without risk, and effectually. The mode now adopted for this purpose differed entirely from what was called "forcing down a stricture" by the introduction of a full-sized catheter. This was not unattended with danger; for it was not always possible to tell where the point of the instrument would go, or to what extent the urethra might be damaged. By the mode employed by either Mr. Thompson or Mr. Holt, no such risk occurred. He (Mr. Fergusson) thought the plan mentioned by Mr. Solly would not be found so effective as when the silver instruments were used. With regard to Mr. Thompson's instrument exhibited that evening, he questioned if it were strong enough to break up the stricture without breaking the instrument in some of the more severe cases. This could not occur when Mr. Holt's plan was resorted to: however great the force applied, the instrument could not give way. So much force was occasionally required that he (Mr. Fergusson) should be fearful of trusting to Mr. Thompson's instrument. This instrument, as far as the plan of treatment itself was concerned, did not differ much from that of Mr. Holt. The plan of immediate dilatation would do much towards abolishing internal and external urethrotomy.

Mr. Acton was glad to hear that the advocates of the various plans for treating severe forms of stricture had not found cause for disparaging that for internal incision, a treatment he had found most successful. It was admitted on all hands that dilatation, consistently persevered in, was alone sufficient to remedy the greater portion of strictures. In hospital practice (in order to save the time of the poor) it might be necessary to resort at once to other plans; but among the upper classes, where a rapid cure was not of such importance, the patient usually objected to have his stricture split, or any other similar heroic remedy employed, and which he (Mr. Acton) was glad to hear was unattended with any danger. The Fellows of the Society, must, however, recollect—and the shelves of the surrounding library would support the statement—that the advocates of every novel treatment of stricture had ever lavished the same praises on their different systems as had been listened to that evening. Time could alone test the value of the rival schemes; but he was not inclined to think that any one system would be found applicable to all cases. It had been stated in the course of the discussion, that within a short period, and in the practice of one surgeon, 250 cases of stricture had been split. Now, seeing the comparative rarity of cases requiring such treatment, we must believe that many strictures had been split which would have been treated (by other surgeons) by more simple means. The author of the paper had spoken of the success of his treatment depending "upon over-distension of the fibrous tissue, so as to destroy, or at all events impair, its natural tendency to contract." He (Mr. Acton) was yet to be convinced that over-distension at one sitting would accomplish this very desirable end; for the more he treated stricture, and the more he studied the structure which composed the indiarubber-like mass we had to distend, the less disposed was he to believe in a rapid cure, or that a healthy structure would at once displace this elastic tissue. Stress had been laid during the discussion by more than one speaker on the advantages of his or their operation as succeeding after others had been tried and failed. Mr. Acton would appeal to the practical Fellows of the Society if cases were not brought under their notice of patients who had been condemned to these heroic operations, and who before submitting to them applied for a second opinion, and who were cured by dilatation alone, showing that the operation that had been recommended was at least unnecessary. When dilatation was unable to effect a cure, internal incision came in most opportunely, and the division of the elastic tissue by the knife enabled the surgeon to pass his instrument, and cure the stricture. This, he maintained, was the more scientific treatment, and more in accordance with all the theoretical and practical principles of surgery, and one which had been gradually gaining ground since the instruments employed had been perfected.

Mr. Thompson replied *seriatim* to objections raised by Mr. Holt. First, that there was no difficulty in measuring the distance at which a stricture was situated from the meatus in order to adjust his instrument, and that no man who found that a difficulty was competent to use an instrument in the urethra at all. It was, indeed, an ill compliment to surgeons to imagine want of ability to accomplish so easy a matter. Secondly, that it was not often necessary to apply the instrument for two strictures in the same urethra, but if there were two, no difficulty in applying it twice existed. Thirdly, that although it had been his practice to tie in a gum catheter twenty-four hours afterwards, he did not regard it as necessary; he had done so as a precaution, and if any objection to this existed, it would be as safe to dispense with it here as after Mr. Holt's operation. From the remark, that there was no advantage in distending to No. 16, if the bougies subsequently passed could not exceed No. 12, he differed very widely. It had long been admitted to be a desideratum to find some means of dilating the stricture to the size of the canal where it is situated, and it is notorious that the bulbous portion, where stricture most commonly exists, is little more than half dilated by a bougie which fills the external meatus. The subsequent dilatation by such a bougie sufficed, but if thought necessary it might be maintained by distending the instrument itself, which formed an excellent dilator. He had a patient at this moment who preferred it to a bougie for ordinary use. Again, it had been assumed on theoretical grounds that its power was deficient. He could only say that in practice the instrument had resisted very severe tests, and was amply strong enough for its purpose; but he would say again, as he had said in the paper, that where the induration surrounding the urethra was very considerable, involving, possibly, the substance of the corpus spongiosum throughout, especially when anterior to the scrotum, incision was more likely to be permanent in its effects than either his own or Mr. Holt's method. He agreed with Mr. Acton that this was highly useful in some cases. An important distinction existed between forcible catheterism and distension, which had been well drawn by Mr. Fergusson: the dangers of that now happily exploded method resulted from tearing away the stricture from its connexions, and driving it down the urethra. Simple expansion from within outwards, provided it was thoroughly efficient, had now been proved to be unattended with danger. Finally, it was most satisfactory to him to hear from Mr. Holt and others the success which had attended his operation, because in some particulars it was closely allied to his own proceeding: thus, he contended that Mr. Holt's operation might be perfectly performed with his (Mr. Thompson's) instrument, but that the latter possessed in addition these two advantages—first, power to carry the distension to a much higher point; and, secondly, that it could be done gradually and slowly, so as to over-distend the tissues as much, and rupture them as little, as possible. It was on these two grounds of difference that he claimed for this proceeding an examination and a trial, since he conceived them to constitute an improvement of no mean value.—*Lancet, May* 9, 1863, *p.* 522.

61.—LITHOTOMY IN THE FEMALE.

By James R. Lane, Esq., Surgeon to St. Mary's Hospital.

[The operation proposed and carried out by Mr. Lane is not by any means devoid of interest and novelty, although not a new idea. The case in which the following operation was performed (extraction of the stone by an incision into the base of the bladder through the vesico-vaginal septum, followed by immediate closure of the wound) occurred in October, last year, and was in a woman aged thirty-eight, who had suffered from stone many years.]

The operation was performed in the following manner :—The patient being placed in the lithotomy position, Bozeman's speculum was introduced into the vagina, and held so as to expose its anterior wall, and a straight grooved staff was passed into the bladder by the urethra. An incision was then made into the groove of the staff, commencing just behind the neck of the bladder, and extending backwards in the median line for about an inch and three-quarters—to within a short distance, in fact, of the attachment of the vagina to the cervix uteri. Through this incision the stone, which was nearly two inches in length, was readily extracted. The edges of the wound were then brought into apposition by twelve silver wire sutures.

Immediate relief of all the painful symptoms followed the operation; the greater part of the incision united by first intention, but, unfortunately, a small portion of its edges at the anterior part sloughed, and when the slough separated the urine made its way into the vagina. This opening, however, healed by granulation, the granulating surfaces being held together by the insertion of two wire sutures. It remained closed for a fortnight, the patient being able to retain half a pint of urine without difficulty; but at the end of this time, the uniting medium, which was at one point very thin, gave way, and an opening was formed just large enough to admit a probe. Failing to close this by repeated applications of caustic, I pared its edges, and again applied sutures, this time with complete success, and the patient shortly afterwards left the hospital quite well, having perfect control over her urine, and all inflammation and irritability of the bladder having subsided, the vesico-vaginal septum remaining as thick and strong along the line of the incision as elsewhere.

Vesico-vaginal lithotomy is no new thing, but it has never taken its place as an approved and recognised operation, in consequence of the great difficulty of securing the closure of the wound, and its being consequently so often followed by the distressing condition of a vesico-vaginal fistula. A French surgeon named Rousset has the credit of being the first to perform it, in a case in which the bladder, pushing before it the upper wall of the vagina, protruded through the vulva. Fabricius Hildanus, following the example of Rousset, practised it in two cases, in one of which the stone had partially perforated the vesico-vaginal septum, while in the other there was a vesico-vaginal fistula following parturition. In 1740, Gooch extracted a calculus through the vagina weighing three ounces, which had caused ulceration through the septum. M. Velpeau has given the names of various operators who have since, at different times, adopted the vesico-vaginal method of performing lithotomy; and, amongst others, M. Faure, who made his incision in the median line, but very obliquely through the septum; that is to say, directing the edge of his knife towards either the right or the left side, so as to produce a broader cut surface, and to give the wound somewhat of a valvular arrangement. He thus, in 1808, succeeded in curing his patient without the formation of a fistula.

Most of the earlier operators speak only of a removal of the stone, and say nothing about the result as regards the formation of a fistula. Chelius even, as recently as twenty years ago, says that "the production of a vesico-vaginal fistula, which has been objected to this operation, cannot be considered as the usual consequence." He advocates it as causing no bleeding and no incontinence of urine, while it affords facilities for the removal of very large stones. Velpeau's conclusion, after looking into the cases recorded by different operators, was not so favourable as this; he thinks that a fistula is met with in at least one case out of four. Both statements, however, there can be no doubt, are far too favourable; and if no steps are taken for the immediate closure of the wound, I fear a fistula must always be reckoned upon as the rule, and its absence the exception. Indeed, the general conviction that this is the truth has caused the vesico-vaginal method to be always regarded with disfavour, and to be reserved as a kind of last resort, for exceptional cases, and where the stone was very large. Velpeau, even with his too favourable estimate, is so impressed with the dread of fistula, that he recommends in preference the high or supra-pubic operation, notwithstanding its greater danger. M. Coste, many years ago, suggested the reunion of the wound by suture immediately after the extraction of the stone, but appears to have been deterred from attempting it by the *then* almost invariably unfavourable termination of operations for vesico-vaginal fistula.

Now, however, all this is changed. The experience of the last ten years has abundantly shown that almost every case of vesico-vaginal fistula, even when attended with great loss of substance, may be firmly and permanently closed by the improved plastic procedures now in use. We may therefore very fairly feel confidence in our ability to close the clean longitudinal incision made for the extraction of a stone, which is attended by no loss of substance, and with no cicatricial contraction or induration.

The merit of first carrying into effect the idea of immediate closure belongs, I believe, to M. Vallet, of Orleans, who published a paper on the subject in the "Mémoires de la Société d'Agric. Science, &c., d'Orleans," t. ii., 1856. I have not seen this memoir, but it is referred to by M. Nélaton, from whom I gather that M. Vallet operated in two cases, the second of which required a reapplication of sutures for the closure of a portion of the wound. I presume from this, that in the first case, the whole of the wound united at once. M. Vallet made a transverse instead of a longitudinal incision, thinking he could select a part of the base of the bladder where the septum was thicker than elsewhere. The septum, however, appears to me so nearly uniform in thickness throughout, that I do not believe any advantage worth mentioning is to be gained by a transverse incision, which might injure the entrance of the ureters, and would be more likely to be attended with hemorrhage, and also to implicate the loose cellular tissue surrounding the pelvic viscera.

Mr. Baker Brown is strongly in favour of the vaginal incision, intending, of course, that it should be immediately closed by suture. He mentions a case in which Dr. Marion Sims adopted it, but does not state with what result.

Vaginal lithotomy, immediately followed by suture, possesses, in my opinion, many and great advantages. Perhaps there is no part of the vesical parietes which may be incised with so little risk. The incision being in the median line, no vessels of any magnitude are likely to be wounded. Vesico-vaginal septum is composed of dense tissue, little, if at all, liable to urinary infiltration, and the free outlet provided for the urine would, even under other circumstances, render such an occurrence improbable. The chance of pelvic cellulitis is, therefore, remote. The peritoneum is not likely to be injured, for even if the incision required to be carried as far back as the attachment of the vagina to the cervix uteri, the vesico-uterine reflection of the peritoneum is situated nearly an inch higher up. Velpeau, in his inquiry into the history of this operation, seems to have been struck with its great immunity from danger, and with the slight degree of constitutional disturbance which it entails ; in fact, he did not meet with the record of a single fatal case.

The favourable progress of my own case was certainly remarkable. Almost from the moment of the operation the pain began to subside, and by the next day had completely disappeared. The slight feverish excitement which had resulted from the attempt at lithotrity also rapidly passed away. The urine, which had been muco-purulent and phosphatic, in a day or two became clear. There was never any pain worthy of remark in the region of the wound, even though a small portion of it sloughed. What uneasiness existed, was caused, after a few days, by the continued presence of a catheter in the urethra. The effect of the removal of the foreign body, and of the draining away of the urine through the catheter, afforded an excellent exemplification of what "physiological rest" is capable of doing for the relief of an inflamed and irritated organ.

As far as I have been able to ascertain, this is the first case in which the operation by vaginal incision, with immediate closure by suture, has been practised in this country. I find, however, that its expediency had some time previously been fully recognised by Mr. Paget, of Leicester, as shown by a paper read to the British Medical Association in June, 1862; but the mode of procedure adopted was, in my opinion, faulty in several important particulars, and was not likely to be followed by very satisfactory results. Mr. Paget's first operation was performed in Sept., 1859, on a child aged three years and a half —an age but ill adapted for manipulation in the interior of the vagina, the size of which must render it almost impossible to apply the necessary sutures. Again, the incision was made through the whole length of the urethra, beginning at the meatus. On attempting to seize the eges of the divided urethra, they had so completely collapsed, and the bladder itself had so retracted within the small vagina, that after several trials the operator was compelled to desist, and leave the result to Nature's unaided powers of repair. That result was a permanent incontinence of urine. Mr. Paget operated a second time, in January, 1861, on a patient aged eighteen, making the same incision, involving the whole length of the urethra. The edges were brought together with five wire sutures. The wound healed, excepting at its anterior parts, which circumstance gave the meatus the appearance of being larger than natural. This patient appears to have ultimately recovered the control over her urine, but was troubled with more or less of incontinence for some months.

I do not understand the object of commencing the incision at the external orifice of the urethra; indeed, I think it cannot be too strongly urged that this canal should on no account be interfered with. The incision should commence just behind the neck of the bladder; that is to say, in an adult person at least an inch and a quarter behind the external orifice of the urethra. Incision of the urethra gives no additional room, but increases the length of wound without any compensating advantage whatever, and the urethral portion of the wound is incomparably more difficult to close than that which is deeper in the vagina.

Mr. Fergusson, in March, 1862, (see the Lancet, Oct. 11th, p. 388), operated in King's College Hospital on a child, aged nine years and a half. He commenced his incision immediately behind the neck of the bladder, and extracted the stone without difficulty. The wound was three-quarters of an inch in length, and its edges were brought together with *one* silver-wire suture, which was removed the next day. The wound did not close. In April, 1862, Mr. Fergusson operated on a patient aged twenty-one, by slitting up the urethra in a direction downwards and outwards, and dilating the neck of the bladder with his finger. No sutures were applied in this case. Incontinence of urine was the result.

Dr. Lyon, of Glasgow, in Sept., 1862, (vide the Lancet Nov. 1st), removed a stone by vaginal incision in a patient aged forty-two, immediately closing the wound by sutures, and causing all the precautions necessary to ensure success. A small portion of the wound did not immediately heal, but closed in a few days by granulation. In this case some hemorrhage took place into the bladder the evening after the operation, and was not suppressed until a saturated solution of alum had been injected.

In August, 1862, Dr. Robert Nelson performed a similar operation, and with perfect success. (See American Medical Monthly Journal, Sept. 1862).

These are the principal facts relating to this operation that I have been able to collect. They are not numerous, but I cannot help thinking that they are very suggestive. Each time that the operation was performed in an appropriate case, and conducted upon proper principles, complete success was obtained; while in every instance of failure, the result may, I think, fairly be attributed either to the injudicious selection of the case, or to the manner in which the details of the operation were carried out. The conclusion which I have formed is that in an adult female, and especially in the case of a large stone, lithotomy through the vagina, conducted on proper principles, and followed by immediate closure of the wound, is the safest and best procedure that has as yet been devised, and deserves to be accepted as a recognised operation in surgery. It should be understood, however, that it is not applicable to children; neither is it well adapted for young unmarried women, in whom the difficulties of this operation must necessarily be greatly increased.—*Lancet, Jan.* 10, 1863, *p.* 34.

62.—ON VAGINAL LITHOTOMY.

By Dr. Aveling, of Sheffield, and T. Spencer Wells, Esq.

The paper (read by Dr. Aveling before the Obstetrical Society of London) commenced by reciting the particulars of thirty-five cases in which this operation had been performed—twelve British, and twenty-two foreign. The author also gave another case, in which he divided the vesico-vaginal septum, and extracted a small rough stone. The wound was brought together with silver-wire sutures. Gilt beads were passed over the ends of these, and run down to the lips of the wound. These were kept in position by a perforated shot, also passed over the ends of the sutures, and tightened upon them by a pair of forceps. He proposes in future to use a coil, made by winding a piece of the suture wire round a pin, instead of the beads. The wound healed in a week, and the patient returned to her home in a fortnight.

Mr. Spencer Wells congratulated Dr. Aveling upon the successful result of his interesting case, and heartily concurred in the tribute he had paid to the services of Dr. Marion Sims. But he (Mr. Wells) had begun to doubt whether the success which had followed the operation for the cure of vesico-vaginal fistula of late years was so much due to the use of wire sutures, as to the improvements which Dr. Sims had originated in the mode of bringing the fistula into view, accurately paring the edges, and bringing them into perfect apposition. Provided the edges of a fistula were thoroughly pared, and kept in close apposition, it was probably of little importance how this was done. A year ago he (Mr. Wells) was as strongly in favour of metallic sutures as anybody; but latterly a wider experience had taught him that it is only after five or six days that wires show any advantage over silk, and before that time the sutures ought to be removed. Then silk offers the great advantages over wire of being more easily applied, of not requiring so large a needle to pass it, of the ends being much less irritating, and of being more easily removed. After many comparative trials on different parts of the same wound with wires of silver, iron, lead, platinum, and aluminum, and with fine catgut, horsehair, telegraph wire, india-rubber thread, and the fine strong silk known as "Chinese twist," he had become convinced that wires offered no advantage over silk, while silk offered many advantages over every other material used for sutures. In a recent case he had closed a vesico-vaginal fistula by five silk sutures, and perfect union resulted, although no catheter was used. The supposed necessity for the use of the catheter after closing vaginal fistulæ, was another error which time was correcting. The urine is by no means so irritating a fluid as some believe. The lower orders use it as a lotion to the eyes and to sore legs; and it certainly cannot differ much from the dilute saline solutions constantly prescribed as astringents or stimulants. The use of the catheter is the most troublesome part of the after-treatment, and often most distressing to the patient. One of his patients really could not bear it, yet she did perfectly well; and lately he had not used it at all, union taking place quite as well as when it was used, and the patient being much more comfortable. With regard to stone in the bladder during labour being a cause of vesico-vaginal fistula, he had once removed in the Samaritan Hospital a large stone through a fistula before closing it; but it was very questionable whether it could often be necessary to remove a calculus through the vagina when no fistula existed, or to run the risk of making a fistula to remove a stone. Lithotrity was very easily performed in women; and large fragments of stone passed readily through the short female urethra, so that no form of lithotomy could often be called for. Simple dilatation of the urethra was not likely to answer in any case not suitable for lithotrity, and its effects are very uncertain—a large stone might be removed and no incontinence follow; but incontinence might follow removal of a very small calculus. The usual aid to dilatation by incising the urethra was still worse. A surgeon of very large experience had told him that he had done it for two adults and seven children, and "they were all dribblers." Where, from some exceptional condition of bladder or stone, lithotrity was inappropriate, vaginal lithotomy might, therefore, become a valuable operation; but experience was still wanting to show that it was better than, or as good as, the lateral operation so successfully practised by Dr. Buchanan, of Glasgow. The subject was a comparatively new one, and Dr. Aveling deserved the thanks of the profession for the light he had thrown upon it.—*Lancet, Jan.* 31, 1863, *p.* 119.

DISEASES OF THE SKIN.

63.—TREATMENT OF RINGWORM.

By Dr Alexander Lane, R.N., Nova Scotia.

For the cure of ringworm, I take two drachms of pure iodine, and rub down with it in one ounce and a half of spirits of wine as much of the pure hydriodate of potash as will dissolve it. I paint the parts affected with this by means of a camel-hair pencil every two hours until the parts become quite black, extending the application one quarter of an inch over the edge of the parts affected, and at each application you will have to rub it over half-a-dozen times or more. This takes off the skin in large flakes, as the case may be. I continue the application until I have a perfectly new skin formed. I have never known it fail when properly used and proper attention paid. The parts should be excluded from the atmosphere by means of adhesive plaster.—*Dublin Medical Press, Dec.* 3, 1862, *p.* 562.

64.—ON THE DIAGNOSIS OF ECZEMA.

By Professor Hebra.

Professor Hebra attaches a different meaning to the term eczema from that which has been generally accepted. He does not restrict the term to a disease characterised by the formation of vesicles followed by desquamation of the epidermis; but includes in it all disorders of the skin which are attended in either their earlier or their later stages by the presence of vesicles and serous discharge.

Eczema may be produced by artificial irritation of the skin. In this case, however, the results are not only vesicles and serum, but sometimes also mere redness and desquamation, sometimes small papular elevations of the size of pin's heads, sometimes even pustules and scabs. Again there are a considerable number of cases in which the same person has on one part of his body desquamation from a reddened surface, on another red elevations as large as millet-seeds, and on another epidermic elevations filled with watery fluid; while other parts of the skin are partially denuded of their epidermis, and moistened with exudation. There may also be here and there yellow purulent points, of which some afterwards form brown or green scabs. Thirdly, the observation of the course of individual cases of eczema shows that many begin with the development of large and small vesicles, of which some become pustules, others burst, leaving exuding spots, while others are covered with yellow scabs, around which are formed papules and red desquamative spots. Towards the close of the disease, the whole of the pustules have formed scabs, which partly or entirely fall off after the drying of the exuded fluid, leaving red spots, more or less infiltrated, and covered with fine scales.

These facts Professor Hebra believes sufficient, in conjunction with clinical observation, to support the conclusion that the term eczema includes five distinct forms of disease. In support of this idea, he brings forward the following considerations.

1. The identity of the different forms of eczematous eruption may be proved by a simple experiment, which any one may make on himself or on persons at his disposal. Let an agent capable of producing artificial eczema (such as croton oil) be rubbed into corresponding parts of the skin of two persons, or into different parts of the skin of the same individual; the same quantity—say five drops—being applied to each part. In a few hours the effects will be manifested, but differently in different parts. On the scrotum and penis there will be œdematous swelling and redness, accompanied in many cases by innumerable vesicles; on the face the swelling will be more marked and the vesiculation less; while on the skin of the extremities the hair-follicles are swollen and raised above the level of the skin, and red elevations appear also here and there. If the application be not repeated, the swelling, vesicles, and red elevations disappear in a few days, leaving only slight redness and desquamation as indications of the inflammatory process that has been set up. But if the croton oil be again applied to the same parts on two, three, or more days in succession, the disease produced is increased in intensity as well as in extent. The eruption on the parts themselves is increased, the red elevations being increased in number, and forming vesicles in consequence of the greater amount of sub-epidermic exudation; and the parts surrounding those in which the application has been made also become covered with an efflorescence similar to that which followed the application made on the first day. When artificial eczema has reached this degree of intensity, it does not end in simple desquamation; but in most cases the vesicles burst, and leave exuding spots; while the contents of other vesicles become purulent. As a consequence of the formation of the pustules, the skin surrounding them becomes more red, swollen and painful. Having reached this point, the eruption recedes. The vesicles and pustules gradually dry and form scabs, which are thrown off by the new epidermis, leaving red patches, more or less infiltrated, and covered with desquamation.

If these results of the application of croton oil be analysed, they will be found reducible to five principal forms; viz., red elevations and vesicles, produced by the first application; red exuding patches produced by the continued operation of the same irritation; pustules and crusts, arising from the metamorphosis of the elevations and vesicles; and finally, the red desquamating patches left after the removal of the other forms of efflorescence. These conditions are represented in several species of eczema to which authors have given names, and which Professor Hebra thus arranges in the order of their intensity: 1. Eczema squamosum or Pityriasis rubra; 2. Eczema papulosum, also called Eczema lichenoides and Lichen eczematodes; 3. Eczema vesiculosum—the Eczema solare of Willan; 4. Eczema rubrum seu madidans; 5. Eczema impetiginosum—Eczema crustosum of some authors.

Whether the same irritant will produce the same form of eczema, depends on the quantity and strength of the application, as well as on its duration. Differences arise also from the part to which the irritant is applied—the skin of the genital organs, the face, and the flexures of the joints, being most susceptible, and from the state of health of the individual; it being often impossible to produce an eruption by irritating the skin of a person in health, while, if he fall into a diseased state, an eruption is readily called forth.

2. When eczema is spread over the scalp, face, trunk, and extremities, either continuously or in patches, it rarely happens that the same form of eczema is developed on each part. For example, the scalp and the skin of the face may be the seat of impetiginous eczema, while the integument of the muscles of the ears, the nape of the neck, the axilla, and the flexures of the joints, are covered with eczema rubrum, the limbs with eczema papulosum and vesiculosum, and the trunk with eczema squamosum. It is more reasonable, Professor Hebra holds, to regard these as forms of one and the same disease, than to diagnose the eruption on the scalp as porrigo or tinea mucosa or granulata, and that on the face as porrigo larvalis, impetigo faciei rubra, crusta lactea, crusta serpiginosa, or melitagra flavescens and nigricans; while the word eczema is confined to the vesicular exudative eruption on the trunk and extremities. The terms impetigo and pityriasis rubra he regards also as having been adopted without reference to the eczematous appearances preceding the conditions which have received these designations.

3. Clinical observation affords the most satisfactory confirmation of the view here adduced. In one case eczema begins with the eruption of vesicles; in another the formation of vesicles is preceded by the appearance of red desquamative spots or papules. Or both papules and vesicles may be simultaneously developed; and of the latter, most rapidly form pustules, the contents of which as rapidly dry into yellow crusts. Hence it is also evident that vesicles cannot

be regarded as the sole form in which eczema at first appears. The appearance of the eczema also undergoes changes in the course of the disease. After the falling off of the crusts, the appearance known as eczema rubrum may be left; while, where vesicles have been at first developed, the outer layers of the epidermis are thrown off, leaving the rete mucosum denuded and throwing out exudation. Again, when the exudation ceases, the parts pass into the infiltrated, red, desquamating phase, known as eczema squamosum or pityriasis rubra. Professor Hebra observes, in conclusion, that this view of the metamorphosis of eczema is more probable than the idea which some have held of the conversion of one disease into another—of eczema into impetigo, porrigo, tinea, pityriasis rubra, militagra flavescens, and the like. *Wiener Medizin Wochenschr.—British Medical Journal, March* 7, 1863, *p.* 241.

65.—TREATMENT OF ULCERS OF THE EXTREMITIES BY "SEALING."

(Cases under the care of Barnard Holt, Esq., at the Westminster Hospital.)

In the treatment of ulcers of the legs a novel method has recently been adopted in the Westminster Hospital by Mr. Holt with the best effect. The treatment consists in excluding the air from the wound during the process of granulation, and this plan is found to assist materially in rapid cicatrization, when once a healthy action is set up.

The method of applying the dressing, as practised by Mr. Holt, is as follows:—The margin of the ulcer is covered with adhesive soap-plaister, half an inch wide, and a piece of oil-silk, large enough to cover both the ulcer and the plaister, having been carefully affixed by means of collodion, another edging of plaister is put on the margin. The transparency of the oil-silk allows the progress of the ulcer to be inspected with the greatest ease.

In some clinical remarks upon cases under treatment by this method, Mr. Holt observed, that "To render the treatment effective it was necessary to remove the sealing in accordance with the amount of discharge present. When the discharge is abundant it may be necessary to remove the first sealing on the second day; but experience shows that, as the treatment is proceeded with, so the discharge gradually diminishes in quantity, and that the granulations which, before the sealing, were pale and flabby, become floride and vigorous. Hence, the second application of the sealing may usually be allowed to remain untouched for five or six days, and the third even longer, and so on until cicatrization is complete. No dressing of any kind is required beneath the oil-silk, which should be carefully secured, so as to exclude the air. By this simple method all irritating influences are avoided, the discharge is not too frequently removed, and the growth of healthy granulations is induced, leading to the rapid cicatrization of the ulcer."

We subjoin two cases illustrative of the treatment, for the notes of which we are indebted to Mr. Arthur Beadles, House-Surgeon to the Hospital:—

William J., aged twenty-two, a sailor, admitted, June 24, into Northumberland Ward, under the care of Mr. Holt, with an ulcer of the leg.

History.—About three years since, whilst on board ship, his right leg was crushed by some spars which fell upon it, and, for twelve months after this, pieces of bone came from the wound, which then healed up, and was quite well for six weeks. It then broke out into an ulcer again, and continued to enlarge up to the time of his admission.

On admission there was an inflamed ulcer on the outer side of the right ankle, about two inches long, and one inch wide, but not very deep. There was considerable inflammatory swelling of the surrounding parts, and he complained of burning, pricking pain in the ulcer itself. Ordered a linseed-meal poultice, and, a couple of days afterwards, a nitrate of silver lotion (gr. ij. ad ℥j.)

July 1st. The ulcer was sealed.

4th. Unsealed, and found to be diminished in circumference half-an-inch; re-sealed.

7th. Dressing renewed. Discharges a good deal.

10th. Ulcer much diminished in size.

14th. Ulcer healing rapidly, and of very small size.

25th. Discharged cured.

Martha B., aged twenty-two, a servant, admitted June 24th, into Percy Ward, under the care of Mr. Holt, with ulcers of the legs.

History.—On December 28th last, she received a blow on the left leg, which gathered and was poulticed, and a large slough separated, when lotio nigra was applied. The ulcer continued to increase in size, and an ulcer appeared on the right leg without apparent cause, about three weeks before her admission.

On admission there was an ulcer on each of the legs immediately below the patella. The largest, on the left leg, was of the size of the top of a small teacup, whilst that on the right leg was of the size of a two-shilling-piece. They were both very deep, with ragged edges, and discharged freely. Ordered linseed meal poultice.

June 26th. Ulcers more healthy in appearance, and less painful. ℞. Acidi nitrici dil., ♏xv.; decocti cinchonæ, ℥j., ter die.

27th. The depth of the ulcers much decreased, and their size diminished. The one on the left leg is about the size of a five-shilling-piece, and that on the right about the size of a shilling. Both ulcers were "sealed" according to Mr. Holt's method.

30th. Left ulcer of the size of half-a-crown, and the right of a sixpence. Ulcers re-sealed.

July 10th. Ulcers unsealed, and found to be much smaller. The same dressing applied.

14th. The left ulcer of the size of a shilling, and the right completely healed.

29th. Discharged.—*Med. Times and Gazette, Dec.* 6, 1862, *p.* 601.

66.—ON THE PREVENTION OF PITTING IN SMALL-POX.

While vaccination is generally regarded as the grand preventive of the disease, and all but universally practised, it has long been felt that medical men would confer a great boon on society if they could discover some means by which the disfigurement of the face could be prevented. We believe that, by a very simple application, this desirable end has been attained in the clinical wards in the Royal Infirmary; and it is in the hope that when known it may be generally practised that we at present draw attention to it. The application consists of a solution of india-rubber in chloroform, which is painted over the face (and neck in women) when the eruption has become fully developed. When the chloroform has evaporated, which it very readily does, there is left a thin elastic film of india-rubber over the face. This the patient feels to be rather comfortable than otherwise, inasmuch as the disagreeable itchiness, so generally complained of, is almost entirely removed, and, what is more important, "pitting" once so common, and even now far from rare, is thoroughly prevented wherever the solution has been applied. It may be as well to state that india-rubber is far from being very soluble in chloroform, so that, in making the solution, the india-rubber must be cut into small pieces, and chloroform added till it is dissolved. The medical gentleman who has introduced this treatment has tried several other substances, but found none so generally useful. For instance, gutta percha was tried. It has the advantage of being very soluble in chloroform, and would have been a very admirable application but for the tendency it has to tear into ribands whenever the mouth is used, or even when the features play. India-rubber, on the other hand, is pliable and elastic, allowing free use of the mouth without any danger (as a rule) of its tearing off. If, however, from some cause or other, a portion is torn off, a fresh application of the solution by means of a large hair pencil remedies the defect, and the mask is once more complete. Several patients who have had this india-rubber mask applied concur in stating that they found it agreeable to wear, and their faces were perfectly free from "pitting," although other parts of the body, such as the arms, were covered. The credit of this valuable invention and application belongs to Dr. Smart, house physician, clinical wards, Royal Infirmary; and, while he no doubt in the proper quarter will make good his claim to the honour, he will feel amply repaid by its general adoption by his medical brethren, and the consciousness that he has done something to increase the resources of the medical art.—*Scotsman.*

Dr. GEORGE writes to the *Morning Post:*—In the year 1833 I published an essay on that disease, recommending the use of prepared calamine, upon the same principle on which Dr. Smart recommends the application of a solution of india-rubber; that is, exclusion of atmospheric air. (Griffith and Ferrand, St. Paul's Churchyard.) In a very severe case, which occurred in my practice since the publication of my essay, in which the face and throat were frightfully swollen, I dressed one-half of it with calamine powder, and the other half I pencilled over, using a flat hair pencil, with sweet oil and the white of an egg, in equal parts well mixed, three or four times a day. No solution of india-rubber, or any other substance, would have answered the purpose better; and its application was certainly attended with more comfort than that of the use of the powder; but it is not only the pitting which is prevented by the calamine, but the rescuing the patient from a state of suffering bordering upon misery; for on the tenth day, or thereabouts, the patient has not a sore place on any part of his body, the time when, according to the usual management, his real sufferings begin. Will you permit me to offer a few practical remarks on the treatment of the disease, which might be useful to the public, and which even a nurse might act upon? Firstly, From the commencement of the disease I would cover the whole body, face and all, lightly with the calamine, shaken through a common pepper-box; or, if hair powder be employed, the powder-puff had better be used, taking care that these powders do not remain in masses. The inflammation on each pustule is by these applications much lessened, a point of great consequence. Secondly, Sprinkle about one ounce of powdered camphor every or every two or three nights between the under sheet and blanket, the whole length of the body, putting most about the shoulders and neck. The relief obtained by this, few would credit until they had had experience. Thirdly, in the advanced stage of the disease, should hardened incrustations have formed, they may be removed, and without much pain too; for in one case I removed every portion of the cuticle from the whole face, forehead, and even eyelids, applied the calamine, and in a few days the cuticle was re-formed without a blemish.

67.—ON THE TREATMENT OF BURNS.

[A retired surgeon publishes the following useful and simple treatment of burns.]

I have used a paint composed of chalk, linseed oil, and vinegar in cases of burns and scalds, both in hospital and private practice, for forty years, and I believe that no application can compare with it as regards relief from pain and curative results. I made my first acquaintance with the remedy in this wise:—Among my earliest patients was a poor child, dreadfully and, as I thought, fatally burnt over the chest and throat. I applied the usual *secundum artem* stimulants of that day, and left the child screaming from the torture of their application, giving little hope of its recovery; after a few hours I visited my little patient and found it in a calm sleep, and covered over as far as the extent of the burn by a queer chalky-looking paint. An old woman had applied it, and by so doing had put the orthodox treatment to shame. I submitted the paint to analysis, and it was found to consist of the ingredients I have named. Let any one lightly burn the finger-end and apply this remedy, and he will then have some idea of the relief it is capable of giving in cases of more extensive mischief. I am not disposed to agree with F. C. S., as regards the *modus agendi*, rather considering that the principle by which relief follows its use is twofold—first, by the exclusion of the air, which it effects, as do flour, cotton-wool, and other remedies; and, secondly, by the coolness which it imparts by reason of its partially effervescent state, for it will be found to effervesce on being disturbed, even after the first effervescence has subsided, just as with champagne. Be all this as it may, the relief instantly obtained, and the ultimate curative effects of this simple application, are incontrovertible facts, and in every house the ingredients are at hand. The form for the paint is this:—

Take chalk and linseed or common olive oil, and mix them in such proportions as will produce a compound as thick as thin honey; then add vinegar so as to reduce it to the thickness of treacle; apply with a soft brush or feather, and renew the application from time to time. Each renewal brings fresh relief, and a most grateful coolness. If the injury is severe, especially if it involve the chest, given ten drops of laudanum to an adult, and repeat it in an hour, and again a third time. To a child of ten years give in like manner only three drops; and beware of giving any to an infant. This plan, with an internal stimulant according to age, as brandy or sal-volatile, or both, should be at once adopted, and there need be no impatience for the arrival of the often distant doctor; neither do I advise submission to any change in the plan as regards the chalk paint when he may arrive, for I am quite sure that the College of Surgeons cannot improve it.—*Times, Feb.* 11, 1863.

SYPHILITIC AFFECTIONS.

68.—ON GONORRHŒA AND SYPHILIS.

By Dr. Maurice Henry Collis, Surgeon to the Meath Hospital and County Dublin Infirmary.

[The milder forms of gonorrhœa get well with surprisingly little treatment. It is not necessary to use either cubebs or copaiva, a ready cure being obtainable by less unpleasant remedies.]

In persons of full habit and great vigour I generally give a saline purgative at the commencement, followed up by minute doses of tartar emetic, if there be much constitutional disturbance or a tendency to high local inflammation. Cold affusion will also be of service in these cases. The main treatment, however, is by injections; not heroic solutions of nitrate of silver, which are eminently uncertain and dangerous in their action, but by weak, and frequently repeated, solutions of alum. The rule for applying injections is simple:—If the inflammation be severe, let the solution be weak and frequently used; if it be of chronic type, let the solution be strong and seldom used. This is the key to the successful use of injections; and not only is the rule applicable in gonorrhœa of the urethra and of the conjunctiva, but it may be extended (with necessary modification) to treatment of all inflammations, and to the use of internal remedies as well as external applications. Indeed, I think, a similar rule may be traced in the effects of all stimulating applications, whether general or special, in the use of tonics, astringents, and sedatives; so much so as to convey to my mind the existence of a general law regulating the actions of a large class of remedial bodies. The terms of it would run something in this form :—"The more acute the disease, the more frequent and the weaker the remedy ; the more chronic, the stronger and less frequently applied."

The astringent on which I chiefly rely for the cure of gonorrhœa —is alum. In the most acute form of gonorrhœa, when the discharge is profuse, thick, and glutinous—the lips of the urethra red, villous, and pouting—the patient should be directed to pour a small jug of cold water over the organ, and immediately inject a syringeful of solution of alum of the strength of half a grain to the ounce. This injection is to be repeated every half hour for the first day, and as often at night as the intervals of sleep will allow. In all probability, before twenty-four hours have elapsed, the secretion will be lessened in quantity, and somewhat thinner ; the local heat, swelling, and redness will have abated ; and the ardor urinæ will have almost disappeared, if the saline purgative and the tartar emetic have been used along with the local treatment. The injection may then be increased to a grain to the ounce, and used every hour ; in all probability, after the lapse of forty-eight hours more, the discharge will have ceased entirely ; the injections, however, must be continued for another week or two—but at the strength of half a drachm to the eight ounces—three times a day ; otherwise a relapse may occur, which will be harder to cure than the original clap.—*Dublin Quarterly Journal, Feb.* 1863, *p.* 2.

69.—CHLORATE OF POTASH IN SYPHILITIC SORE THROAT.

By Dr. J. C. Thorowgood, Physician to the Royal General Dispensary.

Some months ago, when at the house of my relative, Mr. Dryland, of Kettering, I was shown a case of most obstinate sore throat, of undoubted syphilitic character, in a gentleman, one of Mr. D.'s patients. The throat was dark in colour, tumid, covered with a whitish secretion, and had slight superficial ulceration at some points : the tone of the voice was strongly nasal, and there was slight deafness. Mercury in various forms had been tried, nitrate of silver had been applied to the throat, and I believe iodide of potassium had been freely given, but no decided improvement had taken place under any of these measures ; and, after persevering with them for several weeks, it struck Mr. Dryland to try the chlorate of potash, and this medicine, he informed me, very soon effected a perfect and permanent cure.—*Medical Times and Gazette, Nov.* 29, 1862, *p.* 592.

70.—ON SYPHILITIC ERUPTIONS.

By VICTOR DE MERIC, Esq., Surgeon to the Royal Free Hospital, and to the German Hospital, Dalston.

[The following paper contains remarks on a very interesting series of phenomena connected with syphilis, namely, those cutaneous manifestations which, after the virus has found its way into the economy, seem to be the result of a vigorous effort of the organism to bring to the surface and cast off principles which are setting up an abnormal action in the system.]

For the medical observer, such eruptions come within the ordinary rules of general pathology; for efforts of a similar kind are observed in various morbid states of the economy. In the exanthemata, in small-pox, in spotted fevers, we notice a vital action of the same kind; but there are in syphilis shades of difference from the diseases just named. In the first instance, we notice that a syphilitic eruption may appear without giving rise to any febrile disturbance whatsoever; such eruption, in the second place, may occur again and again on the same subject; and, in the third place, it may, from the beginning, assume a character of chronicity. Thus we find that cutaneous manifestations in syphilis have characters in common both with fevers and with ordinary affections of the skin. There is, however, on close examination, a great analogy in the causation of fevers and of dermic phenomena; for in both, a dyscrasia is the origin of the symptoms, the difference lying only in the force of the action: sudden in eruptive fevers, slow and more tenacious in apyretic diseases.

Now syphilitic eruptions have this peculiarity: that at different times, in the same individual, they may appear with all the suddenness of an exanthem; and subsequently, when the latter has long disappeared, the same poison may give rise to the slow development of a series of different cutaneous manifestations.

Not so with scarlet fever or small-pox. When the virus of these diseases has once traversed the organism, the latter becomes, as a rule, insusceptible of the action of the same morbid poison. But in syphilis, eruptions may succeed each other; they may or may not increase in severity; and are even observed in the offspring at the very period when the parent, under the influence of a relapse, may present an eruption resembling the cutaneous symptoms apparent in the child.

We are thus forced to admit that syphilitic eruptions must be studied especially and separately, although they in many respects obey general pathological laws. I do not propose to attempt histological and pathological developments to which such a study would naturally lead; but I shall consider syphilitic manifestations on the skin from one single point of view. We will, namely, inquire, in examining these specific eruptions as they come before us in public and private practice, whether syphilis presents, in our times, less intensity than at earlier periods.

A few words touching the diagnosis, etiology, and therapeutics of these eruptions, will, however, be useful before we fairly enter upon our inquiry.

That the diagnosis may sometimes present a little difficulty we all know, nor is this the place for pointing out how such difficulty may be overcome; but I will just mention two cases in point.

A cab-driver, about twenty-five years of age, presented himself, two months ago, in the out patients' room of the Royal Free Hospital. The whole body, and especially the face, was covered with an extremely vivid and fiery-red eruption of large irregular patches running into each other. In some places the patches were a little raised above the level of the skin, and the eyes were highly injected. The penis presented partial phimosis; and, when the prepuce was forcibly retracted, a ragged ulceration, without distinct induration, was observed behind the corona. The glands in the groin were enlarged; but not in the occipital region. The general disturbance was trifling, and the man so cool and collected that I scouted the idea of an ordinary exanthem. The eruption was, however, so general and vivid that I took the patient into the hospital to give him a good chance of recovery. With saline medicines and rest, the eruption, which was merely a couple of days old on admission, had competely vanished in three days; so completely, that not the faintest trace remained. It was now plain (though the diagnosis came rather late) that this man had been attacked with scarlet fever whilst he had a chancre on the glans; and that the febrile disturbance, consequent on such an attack, had remained at a minimum of intensity, this very minimum being, with the existence of the chancre, the cause of the wrong diagnosis.

The patient was soon discharged, and attended subsequently for about six weeks, the treatment being directed to the chancre, which eventually healed.

At about the same period, I admitted a young journeyman baker at the German Hospital, whose eruption presented difficulties of a different kind. He had been treated for chancre under my care, had been discharged when it was all but healed, and had returned to the hospital, after an absence of six weeks, with an eruption which presented almost all the characters of small-pox. The body was covered with umbilicated pustules, in groups and disseminated; in some places the primary papules were still observable, and situated in the very regions where they are mostly seen in small-pox—viz., the axillæ and the nape of the neck. But the complete absence of fever, the clean tongue, and undiminished strength, forced upon myself and colleagues the conviction that the eruption before us was of a syphilitic character. We were, however, a long time in doubt; but, at last, the event proved that we were right.

Another eruption at the same hospital also puzzled us for some time. The man was a tall, half-witted German sugar-baker, who had had some ulcerations on the glans penis, and came to the hospital exactly in the condition represented in a drawing in my possession. Here we had a *bonâ fide* chronic case, with a deceptive syphilitic yellowish tint, and profuse scaling of the epidermis. The doubt went so far that a mercurial treatment was begun, and the patient subsequently had fumigations of calomel without distinct benefit. The disease was unconnected with syphilis, and proved so obstinate that the man left us before he was completely cured.

I have quoted these examples amongst many to show that, in spite of the reliable rules we have for our guidance, the diagnosis, in a few cases, may present much difficulty.

Were, in the next place, the etiology of syphilitic eruptions perfectly reliable, if the cause of such eruptions were ever the same, we should, in certain instances, be less embarrassed; but it is not so. The reliance hitherto placed on the hard base of the chancre has been somewhat shaken; and although such induration, when it has a *bonâ fide* existence, is a certain sign that the organism is infected, it does now and then happen that a soft sore is followed by an eruption, the sore suddenly assuming a certain hardness, just at the period when the symptoms appear on the skin; or the soft sore, as I have seen it several times, may have been long healed when the cutaneous manifestations are observed. Only a few days ago a gentleman returned into the country, after having been under my care for above eight weeks. He was above forty years of age, and had originally called for a mere scratch on the anterior part of the preputial mucous membrane. This had been treated in various ways by the advice of friends, when he came to me, and showed a small superficial sore, on a level with the surrounding parts, free from depth, induration, or the honeycomb fundus of chancre. The glands in the groin were not enlarged, and I treated the case without mercury. The ulceration extended, however, a little; but by dint of cauterization, aromatic wine, and four week's patience, it was just healing, when the patient complained of general uneasiness and restless nights. Three days after this he came with a general papular eruption, which in some places, and especially in the face, was getting pustular.

The chancre had suddenly broken out afresh, had become thick and hard, and seemed largely to partake of the general commotion. This was rather a late induration. We now commenced a mercurial treatment, and I am glad to say that the patient has done well.

As I had made no actual promises, I had not laid myself open to reproaches, and had none to bear. But matters do not always pass off so smoothly. The other day I had the visit of a gentleman about fifty, who had been under the care of an eminent surgeon of this metropolis for a little scratch of the kind I have just described. The patient was distinctly told that the complaint was of a trifling nature, and that no secondaries were to be feared; but scattered patches of psoriasis nevertheless appeared, especially on the palms of the hands, the wrist, and face, and the patient came to me in high dudgeon. He has undergone a mercurial course, is now well, and more calm.

Ricord is fully alive to these peculiarities. Last week, a gentleman who had been a short time ago under his care was sent to me by a friend. He stated that his chancre had been indurated, though all traces of hardness were gone; and he added that for three weeks, Ricord made him come every third day to watch a mere scratch, so as to ascertain whether it was getting hard. Great caution is evidently necessary, and to avoid disappointment we ought to remember that the etiology of syphilitic eruptions is not completely settled. Nor are matters quite clear as yet, in an etiological point of view, whether one eruption can by contact produce another, or give rise to chancre. With vesicular or pustular eruptions, it is no doubt very possible for the secretion to irritate and contaminate the cutaneous surface of another individual; but a certain hesitation is permitted in the case of papules or scaly eruptions. I have for the last twenty years freely handled all kinds of syphilitic eruptions without any detriment, and I do not believe that mere contract, or even friction, can—at least *in dry eruptions*—do harm. I nevertheless think it my duty, whenever my advice is asked, to warn those affected with any syphilitic eruptions from having free sexual intercourse.

Now there is another peculiarity of syphilitic eruptions in an etiological point of view,—namely, the different manifestations on the skin all arising from *one poison*. How is it, in fact, that scaly eruptions, vesicles, tubercles, and pustules will appear upon different patients, nay upon the same patient, in successive crops, although the morbid poison which is the cause of the eruption is ever the same? We are obliged, under these circumstances, to attribute these phenomena to the state of health and strength *in the patient*, to the amount of reaction of which his frame is capable, and the habitual state of his skin. In my endeavours to solve this problem, I have elsewhere proposed a very simple classification of syphilitic eruptions—namely, two great classes, the secreting and not-secreting. In the dry group we naturally have erythema, papules, and scales; and in the moist group, vesicles, pustules, and tubercles.

I am much inclined to the belief that the moist forms appear more frequent upon the weak and debilitated, and the other forms upon those whose powers are more resisting. Indeed, we mostly see pustules and tubercles running into ulceration in patients wanting stamina. It is, in fact, interesting to notice that, from the mildest form of papular erythema, to a very deep, destructive, or even phagedenic ulcer, the process is the same, but becomes exaggerated in a direct ratio with the powers of the organism. If, however, it is true that pustules and ulcers were more formidable a couple of centuries ago, something must be allowed for the freshness of the virus.

One word about the therapeutics of these eruptions. I am aware that the non-mercurial school has numbered most talented men in its ranks; but I do maintain, in spite of the occasional relapses which we have to deplore, in spite of the real and apparent cures which the non-mercurialists have quoted, that mercury is indispensable when the syphilitic eruptions has, *bonâ fide*, taken place. I have administered the metal in hundreds of cases under various forms, and have had the satisfaction, in many, not only to obtain a permanent cure for the patient, but also to render him fit to become the father of a healthy offspring. A few days ago a gentleman called upon me for a trifling ailment. He had undergone two courses of mercury with me, one internally, the other in fumigations. His symptoms, of which psoriasis palmaris was the most conspicuous, all disappeared. He has married, with my full consent, and has two healthy children, the eldest of whom is eighteen months old.

A short time back my esteemed friend, Mr. Henry Lee, and myself, had the satisfaction of curing an extremely severe case of syphilitic tongue by a course of calomel fumigations. The patient, who is now about forty years of age, and very thin, has undergone, for various eruptions, under my care, no less than five mercurial courses in the space of ten years. Fumigations are especially beneficial in annular psoriasis. Very recently a lady, about thirty years old, was quite covered with them; they have almost disappeared after fifteen calomel fumigations. The mercurial vapour acts, in such cases, both locally and generally. As to topical applications, tar ointment is the sheet anchor for the dry forms, and red or white precipitate is very useful in the ulcers which sometimes form after pustules or tubercles.

Let us, therefore, fearlessly, but prudently, use mercury in *bonâ fide* secondary cases. As to the employment of the metal with primary sores, either soft, hard, or half-caste, as they are now called, I must not say a word, as such discussion would allure me from the main subject—viz., syphilitic eruptions.

And now as to the alleged mildness of the syphilitic disease in our days. To form an opinion we should glance at the description given by the authors who wrote in the sixteenth and seventeenth centuries, and then cast a look at the cases which are in our days observed in public and private practice.

The authors who witnessed the first outset of the disease agree in stating that the eruptions were of a fearful character. The pustules grew to a very large size, and often run into phagedenic ulcers, which destroyed vast tracts of surface, and also attacked the bones. We are, however, given to understand that the violence of the symptoms gradually abated, although the disease still made much havoc amongst the people.

Subsequently writers flattered themselves, and Fracartorius was conspicuous amongst them, that the disease was on the decline, and would soon die out. Even Sydenham thought so; but from the descriptions of Hunter and other writers, it is plain that very bad cases were observed at the end of the last and the beginning of this century. I am under the impression that much harm has been done, not only by the reckless administration of mercury, but also by the extensive trials of the non-mercurial system. Nay, it is to be feared that the practice of syphilization, which is somewhat in favour in some parts of Italy, in Sweden, and in Norway, may also cause considerable mischief.

Our methods of treatment are, however, improved, and we no longer whip and banish patients labouring under syphilis; but we still have too frequently cases before us, in private and public practice, which prove that severe pustular eruptions are pretty frequently observed, and that the pustules or tubercles may still run into very dangerous and destructive ulcers.

To illustrate this, I shall just direct attention to a series of cases of syphilis which have come under my care in hospital practice, and arrange them in groups, without entering, for obvious reasons, into the details of every individual case. I have in my possesion coloured drawings, executed by Mr. Dalton, which illustrate very satisfactorily the different kinds of eruptions.

The *first group* comprises such eruptions as are peculiar from their mildness, and which belong to the great class of dry eruptions,—namely, lichen, psoriasis, maculæ, &c. &c. Instead, however, of using these names, it is more convenient to point out these eruptions by the following terms, which include most of the dry forms: small and large papules, scaly patches, and maculæ.

Taking a rough view of these, as to frequency, I would say that they are by far the more common; and that they seldom prove very annoying. They are, however, open to some sources of inconvenience, as they act in so energetic a manner on the rete mucosum that very obstinate yellowish stains may, for months or years, be left on the skin. They may also, if any great disturbance of the organism take place, run into pustules, and thence into ulcers, though this complication is not often observed.

The *second group* contains pustular eruptions. These may be taken as fair samples of the evolution of syphilis upon subjects debilitated from various causes; insufficient nourishment, overwork, defective clothing, or excesses of any kind. Indeed, when the question arises as to the advisability of giving mercury or not for primary sores, we should always keep in view the facts just alluded to. Mercury has a tendency to weaken the organism; but it is at the same time a specific. The advantages and drawbacks are thus pretty equally balanced.

This group comprises pustular eruptions which have formed crusts of various sizes, the breach of surface caused by the breaking of the pustule healing nicely under the crust. When the latter falls off, the stain is often less apparent than in dry eruption. It would seem as if the formation of pus had carried off the colouring matter which, in the dry forms, stains the rete mucosum. The frequency of the eruptions now under notice is, according to my experience, much less than is observed in the dry forms. In pretty healthy subjects the crusts are very small, and soon fall off; in others they reach a large size, and protect the healing process beneath, which begins immediately no more pus is secreted. But in less healthy patients the continuous secretion of pus loosens the crusts; these at length fall off, and lay bare a loss of substance,—namely, an ulcerated surface.

The latter cases form the *third group*, which includes the syphilitic ulcers which I have been able to collect and illustrate. Among these are good examples of phagedenic action without sloughing. The cases included in this group are rare. The patients generally come to us in a pitiable plight, and neglect, connected with want, has a large share in the production of unhealthy and sometimes phagedenic ulcers. I should here mention very distinctly that we very rarely have opportunities of watching such ulcers from the seed (if I may be allowed to say so) to the full fruit. It is thus impossible to say, when first examining an ulcer of this kind, whether it arises from a vesicle, pustule, or tubercle. Each of these may run into ulceration; and not only these, but what is called the gummy tumour may do likewise.

It is stated in books that rupia begins by a vesicle. This probably happens sometimes, but I am sure that it often takes its rise in a pustule. I have in my collection a very fine case of rupia, in which I saw the original pustules.

In the *fourth* and *fifth groups*, containing the ulcers which have run into sloughing phagedena, and the destructive process connected with syphilitic periostitis and osteitis, we find the necrology, as if were, of syphilis. We have no less than six cases of death, which have taken place under my own observation. It is well known that in the Registrar General's reports the cases of death from syphilis mostly refer to infants, It should be recollected that adults may also die from the exhausting effects of certain symptoms of that disease. The picture looks, however, less gloomy when I state that these fatal cases extend over at least six years. But I should not omit to say that I have seen some cases, in private practice, where the patients, from syphilitic exhaustion, went into rapid consumption, which carried them off in a very few months.

One case of this kind I had at the Royal Free Hospital. The patient had several times been under my care for secondary syphilis, and had always been discharged considerably relieved. At last he came in again much exhausted by a harrassing cough. He died soon afterwards, and we found the lungs stuffed with tubercles.

Each of the three fatal cases of sloughing phagedena is illustrated twice; once during the patient's life, and then in the dead-house. I wished these second drawings to be made, so as to preserve a record of the amount of frightful havoc which may be the result of this complaint.

The two other cases of sloughing phagdena recovered, and the drawing shows the peculiar glistening, stellated cicatrix left after these alterations.

The fifth group illustrates the considerable damage which may be done by very severe periostitis and osteitis, the second generally following up the first. Here we have three cases of death, two females and one male; the former from exhaustion, after extensive denudation of the frontal bone, and the latter from an epileptic fit, connected with necrosis of the same bone.

Now, what are we to conclude from the facts just stated? Shall we say that the disease has lost none of its virulence, and that it is as destructive as ever? I think not. I am, on the contrary, convinced that syphilis, judiciously treated, runs in our days a milder course than formerly; that the cases in which great destruction of parts or even death takes place occur in a very small proportion, when compared to the great mass of cases of syphilis; and that they are mostly connected with neglect, want, or excesses. And I would, lastly, very distinctly state that the worst cases I have seen could certainly not be referred to the abuse of mercury.—*Lancet, Nov.* 29, 1862, *p.* 586.

71.—SYPHILIS.

[The following paragraph occurs in a lecture by Dr. Wilks, of Guy's Hospital, on the syphilitic affections of internal organs. It enunciates an important principle in the treatment of syphilis, which we think will at once commend itself as true to the minds of most men who have had much practical experience of the disease.]

In thinking of this subject from a therapeutical point of view, I have long been under the impression that the value of absorbent remedies, as mercury and iodide of potassium, is in proportion to the formation of such low organisable material, and that these remedies are not curative in relation to the syphilitic poison itself; thus the failure of the iodide in secondary syphilis attended only by simple rashes on the skin, but its efficacy where pains in the bones exist, and other symptoms indicative of an inflammation of the fibrous tissues, with a tendency to the production of lymph.—*Edinburgh Medical Journal, Dec.* 1862, *p.* 552.

DISEASES OF THE EYE AND EAR.

72.—ON THE EXTRACTION OF CATARACT.

By Ernest Hart, Esq., Ophthalmic Surgeon to St. Mary's Hospital, Paddington.

[Mr. Hart commences by stating that his experience has led him very decidedly to prefer extraction to any other plan of treatment, whether drilling, discission, or depression of the lens. He believes that these measures may be usefully limited to a very small proportion of the cases in which operation is desirable.]

In order that extraction may be properly appreciated, and attain the full measure of success of which it is capable, it should not be undertaken without very careful preliminary examination of cases; and when performed, there are certain means, in addition to mere manipulative details, by which its success may be greatly aided.

Means of Diagnosis: inutility of the candle test.—In the preliminary examination of cataractous patients there are certain means of investigation which in practice are of very great value; others which have been much vaunted, and still find place in many classical treatises, may now be ordinarily discarded as uncertain and of comparatively little use. Among the latter is the exploration of the eye by passing in front of it the flame of a candle, of which in the healthy eye, with the pupil dilated, three images should be presented. These are commonly called the images of Sanson, although Purkinje had the priority in pointing them out. But any one may convince himself by a little clinical study that this method is not reliable as a means of diagnosis; for the three images may also be seen in an eye which presents a very observable cataract. In very recent times the differential diagnosis of "amaurosis" and cataract has been much discussed: but it would now be impossible for any surgeon who duly avails himself of the existing means of lighting up the interior of the eyeball, to fall into the error of confounding the two conditions. Subjective symptoms, moreover, have not the value which they possessed when our means of examination were less searching; and long-written histories are now little to the point in these cases.

There are three positive means to which I think every practical man may usefully resort in ascertaining the condition of the lens and diagnosing accurately the degree of visual power which its opacity may veil. We may test the transparency or dioptric condition of the lens by the ophthalmoscope, and perhaps then examine the retina, choroid, optic nerve, and vessels at the fundus, in one or the other eye; we may test its reflecting qualities or catoptric condition by causing a concentrated pencil of light to play obliquely on the lens; and where total opacity veils the fundus, we may explore the retina by momentarily making pressure in a given and systematic manner on the ball of the eye, thus testing the possibility of producing the luminous circular images which are excited by this manœuvre where the retina is healthy, and which M. Serres has carefully described as phosphenes or luminous rings of the retina.

To take the simplest of these methods first—that of oblique illumination. It is of course roughly practised by placing the patient near a window having a good light (not a glare of sunbeams), and inspecting the eye carefully sideways, obliquely, and in different positions. But this is quite insufficient, and in this way chromatic changes of the lens have been mistaken for cataract, while incipient cataracts have been overlooked and patients treated for "amaurosis." To employ usefully this mode of exploration, the chamber should be darkened and the face of the patient shaded, while a lamp is placed at his side; the surgeon then, taking the mirror of the ophthalmoscope in his hand, directs a pencil of rays on to the eye; but instead of looking through the central aperture of the mirror, by which he would receive the axial rays of emergence and get an image of the fundus, he looks on all sides of it, and receives the rays obliquely reflected. In this way, or by holding a candle laterally and in front of the eye in the darkened room, and interposing a large lens, he can by successive movements focus the concentrated rays of light upon any part of the surface or depth of the crystalline and its membranes, and can so accurately light it up as to be able to discern the slightest deposit, whether plastic or pigmentary, and to note the earliest traces of central or circumferential opacity. A little practice is necessary in carrying out easily and effectually this method of examining the lens; but when acquired it has peculiar advantages, and is infallible in recognising cataract.

I will not dwell upon the examination of the supposed cataractous eye by the ophthalmoscope further than to say that the examination of both eyes can never be omitted, because this method of examination is capable of affording a great amount of information where the opacity is commencing, partial or incomplete, by revealing the state of the fundus and disclosing such conditions of the retina and choroid (effusion of serum, hemorrhage, optic atrophy, or the like) as may render it imperative to offer a very guarded prognosis, or to dissuade altogether from operation, lest the patient be pained and the operation discredited by failure arising out of conditions which could not be removed by the mere extraction of the lens. In such cases the passage of light *through* the lens often interferes with a clear diagnosis of the nature, site, and full extent of its opacity; and these will always be supplied by the method of oblique illumination to which I have referred. In both, the pupil should be dilated with atropine.

Finally, where the lens is already thoroughly dense when the patient is seen, a most useful resource will be found in the careful examination of the retinal phosphenes by pressure. It is a well-known subjective virtue of the retina that the presence of a solid body in the eye produces a luminous spectrum more or less distinct. This has been carefully studied by Brewster and Serres (d'Uzès). Not to dwell at present upon the interesting theory of the phosphenes, I will only state the practical conclusion of M. Serres, who lays stress upon four phosphenes: the *frontal*, produced by rhythmical pressure over the upper and middle part of the ball, beneath the eyebrows; the *jugal* by similar pressure at the corresponding point below; the *temporal*, over the insertion of the external rectus into the ball; and the *nasal*, over that of the internal rectus. The pressure is made on the eyelid with the tip of the finger, or a rounded ball on a stem; the end of a caustic-holder serves very well. The luminous appearance partakes of the shape of the compressing body (usually circular or crescentic), and is that of a brilliant white flame. When all the phosphenes are produced, the augury as to the healthy condition of the retina is very favourable; and in proportion as they are feeble, or partially present, or altogether absent, is the judgment unfavourable. Of course some little tact and practice is necessary in carrying out the test, but it is one of great value in certain cases only seen in their late stage, and serves to give valuable information, tending to define the utility of extraction in given instances. I recommend it to attention as a useful resource of practice. Some months since, finding the phosphenes perfect, I was able to hold out favourable prospects to a patient brought to me from the country, who was paralysed, and stone blind with cataract in both eyes. Extraction was successful, and he has now very good sight.

When all complications have been carefully examined by these physical means, which may be satisfactorily applied in far shorter time than is occupied by their description, it remains to fix the season for operation, to enter into the medical history and subjective symptoms of the patient, and to determine the the method of after-treatment. As to season, I think it may be considered as generally agreed that the spring months in this climate are the most favourable time, although with proper precautions cataracts may be extracted with success nearly all the year round; and of this there are abundant proofs, so that some are disposed to think that the selection of spring is due rather to popular fancy. Unless the weather be oppressively hot, or peculiarly sharp and severe, either of which extremes is injurious, I believe that the preference for particular months is not based upon any trustworthy series of chemical facts or accurate theory.—*Lancet*, *March* 28, 1863, *p*. 355.

73.—ON STRABISMUS.

By Dr. Soelberg Wells, Ophthalmic Surgeon and Lecturer on Ophthalmic Surgery at the Middlesex Hospital.

[Without describing unnecessarily the various modes of operating for strabismus that have been practised, Dr. Wells passes at once to a consideration of Von Graefe's operation, which is the one generally adopted by him, and the one which he considers the best. The old operation in which the conjunctiva and subconjunctival tissue were widely incised should never be performed.]

The principle of Von Graefe's operation consists in a very careful division of the tendon close to its insertion, with the smallest possible amount of laceration of the subconjunctival tissue, and the tendinous processes of the capsule of Tenon. We diminish the power of the muscle by giving it a more backward insertion; but we, at the same time, preserve its length intact. Our object is only to weaken the muscle, and not to render it more or less impotent. Before proceeding to consider this method of operating, I would, however, recall to your mind the anatomical relations of the muscles of the eye with the ocular sheath. Commencing at the optic foramen, and loosely embracing the optic nerve, the sheath expands, and passes on to the eyeball, which it encloses. It is loosely connected with the sclerotic by connective tissue—so loosely, indeed, as to allow of the free rotations of the globe within it. At the equator of the eyeball it is pierced by the tendons of the oblique muscles, and, more anteriorly, by the tendons of the four recti muscles, with which it becomes blended; being finally lost on, rather than inserted into, the sclerotic, close to the cornea. The posterior portion of the sheath, up to the passage of the tendons, has been called the capsule of Bonnet; the anterior portion, from the passage of the tendons to its insertion into the sclerotic, being designated the capsule of Tenon. On piercing the capsule the tendons of the recti muscles become connected with it by slight cellular processes sent forth from the capsule. These processes prevent the too great retraction of the muscle after the division of its tendon, which would be followed by a great loss of power. It is, therefore, of the utmost consequence that these connecting processes should not be severed by the tendon being divided too far back, or be lacerated by rude and careless manipulations with the squint hook. Von Graefe has, moreover, pointed out that the result may be unfavourable, even although the tendon has been divided anterior to these fibres, for the sheath of the tendon becomes thickened from the point at which it passes through the capsule, and this thickening extends nearly up to its insertion. If the tendon is, therefore, not divided sufficiently close to its insertion, it is apt to retract within this thickened sheath, and this retraction will in many cases prevent its reunion with the sclerotic. In the old operation the muscle was divided far back, frequently even posterior to its passage through the capsule, and it was consequently often rendered so powerless, that the eyeball could not be moved in this direction; its opponent acquired a corresponding preponderance of power, giving but too frequently rise to a secondary squint in the opposite direction. Hence, the popular dread of the operation, "lest the eye should go the other way." But you need not fear such unfortunate results if you practice Von Graefe's operation, if you are careful in its performance, and if you are thoroughly conversant with the theoretical part of the subject. Never do too much, for nothing is so difficult as to retrace your steps, to patch up a fault you have committed. It is far easier subsequently to increase the effect of the operation than to diminish it. Let me impress this strongly upon your minds, for by following this advice you will escape many unpleasant *contretemps.* I know of no surgical operation which is so safe and so sure in its cure as that for strabismus when properly performed. Let us now pass on to the description of Von Graefe's operation.

As it is sometimes very painful, the patient should be placed under the influence of chloroform. The eyelids are to be kept apart by the spring speculum, or, if this proves not sufficiently strong, by the broad silver elevators. An assistant should evert the eye with a pair of forceps (I am supposing that the internal rectus of the right eye is to be operated on), taking care to do so in the horizontal direction, without rotating the eyeball on its axis, otherwise, the horizontal position of the internal rectus will be changed. The operator should then seize, with a pair of finely-pointed forceps, a small, but deep fold of the conjunctiva and subconjunctival tissue, close to the edge of the cornea, and about midway between the centre and lower edge of the insertion of the internal rectus. He next snips this fold with the scissors (which should be bent on the flat, and have tolerably sharp points), and burrowing beneath the subconjunctival tissue in a downward and inward direction, makes a funnel-shaped opening beneath the subconjunctival tissue, this being, however, done very carefully so as not to divide it to too great an extent. If the subconjunctival tissue is thick and strong, it will be better first to take up a small fold of the conjunctiva only, to open this, and then, seizing the subconjunctival tissue, to divide the latter. The squint-hook (which should be bent at a right angle, and have a slightly bulbous point) is then to be passed through the opening to the lower edge of the tendon. Its point being pressed somewhat firmly against the sclerotic, the hook is to be turned on the point and slid upwards beneath the tendon, as close to its insertion as possible, and the whole expanse of the tendon caught up. You must be careful not to direct the point of the hook upwards and outwards, otherwise it may perforate the fibres of the tendon, and only a portion of the latter be caught up; the direction of the point should, therefore, be rather upwards and inwards. When the tendon has been secured on the hook, the conjunctiva which covers its upper portion may be gently pushed off with the points of the scissors, so as to expose the tendon, which is then to be carefully snipped through with the scissors as closely as possible to its insertion. When it has been completely cut through, the conjunctiva is to be slightly elevated on the point of the hook, and a smaller hook passed upwards and downwards to ascertain whether the lateral expansions of the tendon have been divided. Should a few fibres remain, you must divide them, and again ascertain whether any others are still present. Never omit to satisfy yourselves upon this point, for sometimes the lateral expansions are considerable, the tendon spreading out like a fan, and, although a few fibres only might remain undivided, they would often suffice to spoil the effect of the operation.

If you find, on the first introduction of the hook, that it slides up to the edge of the cornea without having caught up the tendon, you may be certain that you have either not divided the subconjunctival tissue at all, or that you have passed the hook between it and the conjunctiva. If the former is the case, you must open the subconjunctival tissue, and then, on reintroducing the hook, you will have no difficulty in finding the tendon. The opening in the conjunctiva and subconjunctival tissue should be but small, and the excursions with the hook limited, otherwise the subconjunctival tissue and the lateral processes of the capsule of Tenon will be extensively lacerated, which will be followed by a too great recession of the muscle.

The after treatment is very simple. The eye, after having been well washed and cleansed of any blood coagula, is to be kept constantly moist with cold water dressing during the day of operation, so as to prevent any extensive effusion of blood under the conjunctiva. No button of granulations will form on the stump of the tendon, if the latter has been divided close to its insertion, and if the opening in the conjunctiva has been made near the upper or lower edge of the tendon, so as not to leave the latter exposed.

The effect upon the squint, which follows immediately upon the operation, will not be the permanent one. We may, indeed, distinguish three stages in the effect produced by the operation:—1st. The period immediately following the operation; 2nd. After three or four days have elapsed; 3rd. After the interval of a few months,—this being the permanent effect. During the first stage the effect will be considerable, for the eye can now only be moved in the direction of the divided muscle by the indirect connexion of the latter with the sclerotic by the lateral processes of the capsule of Tenon. As soon as the divided end of the tendon becomes reunited with the sclerotic, which generally occurs within three or four days, the effect will diminish, for the muscle now exerts again a direct influence upon the eyeball. This is the second stage. But we find that a further alteration in the position generally shows itself a few weeks or months after the operation, the effect being then again somewhat increased. This is due to the action of the opponent muscle, which, on account of its antagonist having been weakened, can now exert a greater influence upon the position of the eyeball.

A clue to the permanent result of the operation is furnished by the position of the operated eye during the accommodative movements of the eyes, when they are directed upon some near object. It is, therefore, of the greatest consequence always to test the position of the eyes during accommodation, immediately after the operation, as soon as the effect of the chloroform has gone off. We have already seen that the position of the squinting eye (convergent strabismus) may vary when the object is approximated closely to the eyes; for whilst the optic axis of the healthy eye remains fixed upon the object, converging the more the nearer the latter is brought, the position of the squinting eye may undergo the following changes:—1st. It may retain its original position, sustaining only a few oscillating, irregular, lateral movements. 2nd. It may remain completely stationary, so that the angle of squinting will diminish the more the nearer the object is brought, until, at a certain point (if the squint be not excessive), its optic axis will also be fixed upon the object, and there will no longer be any squint. If, however, the object is approximated still closer, a divergent squint will arise; for whilst the healthy eye converges still more, the other retains its position, and now deviates (passively) outwards. 3. It retains its position up to a certain point, and then, as the healthy eye moves inwards to follow the object, it makes an associated movement outwards. 4. It deviates suddenly and spasmodically inwards when the object is very closely approximated.

We should, therefore, immediately after the operation, ascertain whether both optic axes can be firmly fixed upon the object, when it is brought to a distance of from four to six inches from the eyes (their state of refraction being normal). If the eyes are very short-sighted, the distance should be still less. The final result of the operation may be predicted from the position which the operated eye now assumes. If it remains stationary when the object is brought up to within eight inches from the eyes, so that a passive divergence will arise on its being approximated still closer, we must expect a certain amount of divergence in the course of a few months. But this will be still more the case if the eye, instead of simply remaining stationary, makes an associated movement outwards. It is necessary to test this at short distances (four to six inches), for the eye might be able momentarily to fix its optic axes upon the object, although quite incapable of maintaining this position for any length of time. In both the above cases the effect of the operation is to be diminished by a conjunctival suture, and particularly so in the latter case. The effect of the suture will vary with its position and with the amount of the conjunctiva embraced in it. Its effect will be greatest if it be inserted in a diagonal direction from downwards and inwards, to upwards and outwards, so that the inner and outer lips of the wound are united. By giving it this direction we also prevent any sinking of the caruncle. The suture diminishes the effect of the operation by re-advancing the tendon, which is closely connected with the conjunctiva and subconjunctival tissue; the divided ends, consequently, will be more closely approximated, and the retraction of the muscle diminished. The suture may remain in for twenty-four to thirty-six hours.

The fourth position which the operated eye may assume during accommodation, viz., making a sudden spasmodic movement inwards, must make us fear that there will be a relapse, —that in the course of a few months the inward squint will again show itself; for this convergent squint which at first only showed itself during accommodation for near objects, will gradually extend also to greater distance. In such cases the operation is said to have been only of temporary benefit; you should, therefore, at once intimate to the friends of the patient that the squint may return, and necessitate another operation.

The extent of the operation must be regulated according to the extent of the squint. I have already pointed out to you how the amount of the latter is to be estimated.

If the squint is but very slight (1 to 1½ line) a partial tenotomy is indicated. We must, however, divide at least three-quarters of the tendon, or even all but a few lateral fibres, otherwise the effect will not be sufficient. Should the latter exceed our wishes it is to be diminished by a suture. If the squint amounts to from 1½ to 2 lines, a careful, complete tenotomy is to be made. The conjunctival opening should be small, and the hook but of moderate size. The accommodative movements must be accurately tested immediately after the operation; for if there is the slightest tendency to divergence, when the object is brought up to 8 or 6 inches from the eye, a suture should be inserted. In a squint of 2 to 2½ lines, the cellular tissue may be somewhat more freely incised, and a larger hook employed. In children we find that the effect is generally more considerable, for the muscle is not fibrous, and the surrounding cellular tissue is very elastic; we may, therefore, in them easily attain an effect of 2½ to 3 lines by a single operation.

If the squint exceeds 2½ to 3 lines, we must always operate upon both eyes. We may perform either a complete tenotomy in the squinting eye and a partial one in the other, or divide the tendon completely in both eyes. In this we must be guided by the amount of squint left after the affected eye has been operated upon. I advise you, as a general rule, never to operate upon both eyes at the same time, except the squint is very considerable, exceeding 4½ to 5 lines. For, if both muscles have been divided at the same time, you cannot accurately test the accommodative movements directly after the operation, and you thus lose the only clue to the permanent effect. It is, therefore, far safer to operate first upon the affected eye, and then, after a few days have elapsed, and the divided tendon has again reunited with the sclerotic, to ascertain how much of the squint is still left. The amount still remaining will guide you as to the extent of the operation necessary upon the healthy eye. If, after having operated upon the latter, you find that the effect somewhat exceeds your wishes, you can always diminish it by a suture. It certainly is far more brilliant to operate upon both eyes at the same time, and thus rid the patient at once of the squint, but then you run the risk of the unpleasant contingency of the eyes subsequently "going the other way." You should always remember that the cure is to be permanent, and not temporary. In some exceptional cases, however, you must run this risk—if, for instance, the time of the patient is limited, or a second visit impossible. If the squint exceeds five lines, you may, particularly in adults, operate safely upon both eyes at the same time. It may be occasionally necessary to operate not only upon both eyes, but even to repeat the operation upon the squinting eye, before we can cure the affection. This generally occurs only in cases of excessive squint, or if the strabismus has existed for a long time, and the muscle has become fibrous. This second operation upon the affected eye requires considerable care, for the effect of the correction will exceed the extent of the retraction, as the influence of the muscle upon the eyeball diminishes in proportion to the backward position of its insertion.

In divergent squint we may be somewhat bolder, particularly as to operating upon both eyes at one sitting. The effect of the operation is generally less, and has often to be increased by subsequent treatment. If we desire an increase in the effect, the patient should be made to look in the opposite direction, which will cause an increased retraction of the divided tendon, so that its reunion will take place further back than would have occurred if the eye had maintained a median position. If the abductor of the right eye has been divided, and we desire to increase the effect of the operation, the patient should be directed to look, as far as possible, towards his left side. The easiest way of attaining this is, by making the patient wear spectacles, the right half of each glass being covered with a piece of court-plaister, for he will in this way be obliged to look to the left. They should be worn during the first three or four days after the operation. Von Graefe points out the fact that, occasionally, though rarely, we meet with cases in which the operation is followed by no effect, either upon the position or mobility of the eyeball, and yet no lateral fibres of the tendon have remained undivided. In such cases there is a second connexion of the muscle with the sclerotic further back, near the equator; in one case, indeed, he found it even posterior to the equator.

If the operation for squint be carefully performed, there is no fear of any but the slightest sinking of the caruncle. A little sinking will occasionally occur, whatever mode of operation be employed; indeed, I know of no method which can guarantee a *perfect* immunity from it. Von Graefe thinks that the sinking does not depend so much upon the gaping of the conjunctival wound, and retraction of its inner lip, as upon the cicatrization of the connective tissue situated between the muscle and conjunctiva, by which the movable caruncle is retracted. The further back this cicatrization extends, the more will the caruncle sink. Hence, the danger of incising the tendon too freely, and of any considerable sweeping about with the hook, and consequent extensive laceration of the subconjunctival tissue.—*Med. Times and Gazette, Jan.* 24, 1863, *p.* 79.

74.—ON THE TREATMENT OF DEEPER-SEATED INFLAMMATION OF THE EYE (SCLEROTITIS AND IRITIS), BY MORPHIA.

By Dr. John Zachariah Lawrence, Surgeon to the Surrey Ophthalmic Hospital, London.

[The inflammation of the eye may for the sake of treatment be divided into three divisions: 1st, of the conjunctiva; 2nd, of the sclerotic and iris; 3rd, of the choroid and retina. Though often blended in one case the symptoms of one of these generally so predominate as to give us the right to refer any given case to one division exclusively. In October, 1858, Mr. Lawrence first prescribed morphia in cases of acute sclerotitis, and consequently observed its remarkable curative influence; and in December of the next year published the interesting paper on the subject to which our readers will do well to refer in Vol. xli. p. 263. This treatment does not do away with the necessity for the use of belladonna externally in some cases (iritis principally). The following cases have occurred to Mr. Lawrence subsequently to the publication of the previous article, and will serve to illustrate the action of the drug in this class of cases.]

Case 1.—*Acute Sclerotitis—Morphia Treatment—Decline of the disease in about four-and-twenty hours.*—S. S., a middle-aged woman, was admitted to the Surrey Ophthalmic Hospital on 3rd November, 1858. The sclerotic was intensely injected, the conjunctiva slightly; the "sclerotic zone" well marked. She suffered such severe shooting pain in the eyeball, eyebrow, and infra-orbital region, as to render her quite sleepless.

3rd November. ℞. Morph. hydrochlor. gr. $\frac{1}{4}$, every third hour. Warm water fomentations to the eye.

6th. Took the morphia regularly up to four P.M. yesterday, when she took the last powder. Towards the evening of the 4th the pain in the eye began to abate; now she feels but a slight aching in the eye on exposure to light. The sclerotic vascularity has considerably diminished. She now recovered rapidly under the treatment of a slight conjunctivitis.

Case 2.—*Acute Sclerotitis—Morphia Treatment—Decline of the Disease in less than twelve hours.*—H. B., an elderly but strong man, admitted to the St. Marylebone Dispensary, 27th July 1859. Sclerotitis of a week's duration characterized by intense vascularity of the sclerotic, and a "sharp, burning" pain in the eyeball and forehead, with nocturnal exacerbation, rendering the patient sleepless. Suffering simultaneously from gout in the great toe. Has done nothing but foment the eye. ℞. Morph. hydrochlor. gr. $\frac{1}{2}$, 3tia, quâque horâ. Warm-water fomentations to the eye.

29th July. Has taken the medicine as prescribed. Slept well, but not heavily, on the night of the 27th after midnight, when the hitherto severe pain in the eye left him. To-day the vascularity of the tunics greatly diminished; the pain in the eyeball, brow, and forehead gone, leaving but a trifling pain at the side of the nose. Bowels have not acted since the 27th. To leave off the morphia and take an ounce of castor-oil.

1st August. Perfectly recovered.

Case 3.—Double Acute Iritis—Failure of Leeching and Mercurialisation—Morphia Treatment—Decline of Disease within four-and-twenty hours.—E. P., was admitted to the Surrey Ophthalmic Hospital on 27th August, 1859. During my absence from town, and up to 10th September, when I first saw her, had been treated by leeching, blistering, mercurialisation, and belladonna lotion for the previous three weeks.

10th September. Iris discoloured; sclerotic deeply injected; pupils dilated (from the belladonna lotion); humours muddy; complains of pain in the eyeballs and eyebrows, "like a rheumatic pain, of an overwhelming weight, of the light causing her great agony;" eyesight very dim. ℞. Morph. hydrochlor. gr. $\frac{1}{2}$, 4ta. quâque horâ. Warm water fomentations to the eyes.

14th. Took the first dose of medicine on the night of the 10th. The pain abated, and she expressly stated "very suddenly." She slept that night. On the following morning she could face the light much better. The medicine has made her feel very sick and drowsy. To-day she complains only of a little "pricking and shooting pain." Her eyes are still dim and weak, but the sclerotic injection is nearly gone.

28th. Since the last report, has been taking the morphia in diminished doses, and subsequently a grain of quinine three times a-day. Her eyes are to all appearance perfectly sound; nothing remains of her disease but a slight haziness of vision.

Case 4.—Acute Sclerotitis—Morphia Treatment—Decline of the Disease in about seven hours.—B. L., aged 40, a working engineer, was admitted to the Surrey Ophthalmic Hospital on 24th September, 1859. Five or six years ago he was struck on the now-inflamed eye by something from a forge-fire. He recovered from the accident in about a month. The eye has been inflamed, as it is now for the last three weeks. It presents all the usual signs of acute sclerotitis; great sclerotic vascularity (the "sclerotic zone" well marked), excessive lachrymation, great pain (especially at night, rendering him sleepless) referred to the eyeball, eyebrow, and temple, and compared by the patient to the sensation of "a weight hanging from his forehead, and pulling him down;" eyesight "foggy;" over the inner part of the cornea a rust-coloured opaque speck, with a minute depression in its centre, evidenced the accident of five years back, but the most careful examination failed to detect any foreign body in the anterior chamber.

24th September. ℞. Morph. hydrochlor. gr. $\frac{1}{2}$, every third hour, watching its effects; warm water fomentations to the eye. Took the first dose about four P.M., felt sleepy about six P.M.; second dose about seven P.M.; the pain began then gradually to "die away;" the third dose about eleven P.M.—slept for three or four hours. The following day (Sunday) at noon, but trifling pain was felt, and he slept soundly that night.

28th. The case is reduced to one of slight conjunctivitis; all pain has left him; found his bowels confined from the medicine. To leave off the morphia, and take a purgative dose of calomel and colocynth, which completed the cure.

Case 5.—Traumatic Acute Sclerotitis—Morphia Treatment—Decline of the Disease in less than four-and-twenty hours.—C. H., aged 46, was on a Thursday evening engaged in Messrs. Maudslay's factory, pouring some molten iron into a sand mould, when a quantity of hot sand flew into his eye. He came to the Surrey Ophthalmic Hospital on Saturday, 1st October, 1859. With the exception of two minute particles of sand, which I removed with a spill of blotting-paper, all the sand had been extracted by one of his fellow-workmen. I found him suffering from intense sclerotitis, marked by universal and high vascularity of the sclerotic and conjunctiva, great lachrymation and excessive pain in the eyeball, compared by the patient to the "prodding of a knife," and rendering him quite sleepless.

1st October. ℞. Morph. hydrochlor. gr. $\frac{1}{2}$, every third hour. Warm water fomentations.

Took the first dose of morphia about three P.M., and then regularly every three hours. It made him feel very drowsy, and that (Saturday) night he slept soundly. The violent pain was entirely gone on the following morning.

3rd. The case reduced to one of a simple conjunctivitis, and treated by a purgative dose of calomel, which completed the cure.

Case 6.—Acute Sclerotitis—Failure of the Morphia Treatment—Recovery under Depletion and Mercurialisation.—E. S., aged 48, applied at the Surrey Ophthalmic Hospital on 12th January, 1859. About twelve months before, she lost the sight of the now-inflamed eye by a blow from the cork of a soda-water bottle. The consequent inflammation of the eye lasted for only a few days; but three or four months afterwards her eyesight began gradually to fade, and she can now only distinguish the outlines (but not the colours) of objects with the injured eye. About three weeks before applying to the hospital she caught cold in the eye, which now presents the following signs:—Intense sclerotic and conjunctival vascularity ("sclerotic zone" well marked); pupil central, of medium size, angular, destitute of contractility. Pain intense, referred to the right eyeball and right side of the head, proceeding from the vertex downwards to the level of the ala nasi.

12th January. ℞. Morph. hydrochl., gr. $\frac{1}{3}$, 4ta quâque horâ. Warm water fomentations to the eye.

15th. Pain and other symptoms unabated. She recovered slowly under leeching, blistering, and mercurialisation.

Whether in this case the deeply diseased state of the eye, or the (too) small doses of the morphia, influenced the failure of the drug, must remain a matter of conjecture.

Case 7.—Acute Sclerotitis—Failure of Counter-irritation—Morphia Treatment—Decline of Inflammation in less than twelve hours.—E. K., æt. 22.—26th August, 1860. Duration seven days. Sclerotic and conjunctival injection; iris unimplicated. Pain in eyeball, supra-orbital, malar, and frontal regions; intense, lancinating, depriving patient of sleep for five nights. Has been blistered on the temple, and used warm fomentations without relief.—℞. Morph. hydrochl. gr. ¼, 3tia quâque horâ. Mitte doses xii.

29th August. Took three doses before bed-time of the 26th; first dose made her feel sick, second relieved, third removed the pain. Slept. Subsequently took all the doses.

To-day she is free from pain and the eye is uninjected. The only symptoms she now suffers from are sickness and faintness (from the morphia). There were soon relieved by an effervescing saline.

[Above twenty more cases are related by Dr. Lawrence in which the morphia was generally successful.]

The following table exhibits the results of the morphia treatment :—

Disease.	Number of Cases.	Treatment successful.	Treatment doubtful.	Treatment failed.
Sclerotitis	24	19	2	3
Iritis	5	4	0	1
Total........	29	23	2	4

In 23 out of the 29 cases the treatment was successful, four being cases (cases 3, 7, 20, and 27) in which ordinary treatment (depletion, &c.) had previously failed. In one of the cases of failure (case 26) there was reason to believe in the patient having, through an error of the dispenser, had calomel instead of morphia powders. A perusal of the preceding cases shows remarkable individual differences in the tolerance for morphia. In a few cases it caused violent symptoms of stomach derangement and depression; in others it produced these effects, but in a slight degree; whilst in others it produced, even in large doses (gr. ⅓), no such effects. The general dose I now give is gr. ¼, cautiously increased to gr. ⅓, or even gr. ½;

but in some young or feeble subjects it is better to begin with gr. $\frac{1}{5}$. I directed the patient to take one dose every third hour; but, as the pain becomes less, to increase the interval of the doses to four or five hours, or even to leave off the medicine altogether. I use the degree of pain as the guide in the length of interval. A mercurial purge and tonics may be required at the termination of the case. Conjunctival complications are treated by the local application of nitrate of silver and atropine drops.

These cases I consider to establish an important practical fact, that morphia is, *per se*, a powerful antiphlogistic, capable of curing those acute inflammations of the eye, in which, up to the present time, blood-letting, blistering, and mercurialisation have been considered necessary. As regards loss of blood, all will be agreed on the propriety of dispensing with it where it can be done with safety. Again, how constant an occurrence is it to see paroxysms of acute inflammations for a time apparently relieved by blood-letting, till the subsequent vascular reaction sets in, but to recur again and again, and to require as many repetitions of the same objectionable remedy? What evidence have we, that, in the combination of mercury and opium with a view of "putting the patient under the influence of mercury," as it is termed, it is not really the *opium* which does the good, and that the mercury and its action on the mouth may not be, to say the least, useless?

If we seek for an explanation of the above very remarkable action of morphia in reducing abnormal fulness of the vessels of the sclerotic, we may find it in the relations of pain to vascular congestion. Pain has generally been regarded rather as the effect than as the cause of the repletion of blood-vessels; but it is quite an open question whether or not, in certain classes of cases, the order of things may not be inverted. Such may be the case in the inflammations of the sclerotic we have just been discussing. That, on the other hand, vascular congestion may react as a cause of pain is not improbable. The theory I would submit is, that the action of morphia in these cases depends on its known power of reducing nervous irritability, which may be viewed as the primary cause of the inflammation. In these deep-seated inflammations of the eye this view is very much borne out by the seat of the pain; this will be found to follow strictly the branches of the fifth nerve; indeed, the precision with which the patients themselves localise the pain is very remarkable, whilst we have further evidence of the nervous nature of these cases in the intense watering of the eye (dependent on irritation of the lachrymal branch of the fifth nerve). In this way I conceive the irritation is propagated to the vessels through the intervention of the connections existing between the fifth and sympathetic nerves.

In instituting such a purely experimental inquiry as the above, it has been necessary not in any away to complicate the treatment; but I am not disposed to ignore in actual practice the occasional value of depletion or counter-irritation; but I must acknowledge my entire scepticism of the "influence of mercury," in the sense in which this term is generally understood. I may further state that now I am convinced of the therapeutic value of my plan of treatment, I generally order the liq. opii sedativ. (Battlei) in combination with an effervescing saline. This very rarely produces any of those unpleasant effects which were often noticed when morphia was given alone.—*Edinburgh Med. Journal, Dec.* 1862, *p.* 492.

75.—ON TRICHIASIS AND ENTROPIUM.

By George Critchett, Esq., Surgeon to the Royal London Ophthalmic Hospital.

By trichiasis we mean an alteration in the growth of one or more of the eyelashes, in such a way and to such an extent that the surface of the eyeball is swept over. Few affections are more embarrassing to the surgeon and distressing to the patient than this disease; for although inverted cilia do not always produce so much irritation in the eye as might be supposed, and the conjunctival surface becomes to a certain extent tolerant of their presence, yet they are both annoying and injurious to the eye. They vary much in number in different cases. I have seen a single fine hair growing in and sweeping the globe, and I have seen cases where apparently an extra row of strong, well-marked lashes have grown from the inner edge of the tarsus directly upon the eye. In a case where one or two fine hairs only are inverted they are difficult to detect, and are often overlooked; and I have met with instances where patients have been subjected to active antiphlogistic treatment for the relief of an ophthalmia that has owed its origin and its persistence to this cause, the existence of which was not suspected by the medical man. I have sometimes met with three or four strong, fully-grown lashes from the upper lid, close to the external angle, and lying within the palpebra upon the globe, and very much concealed. These create much irritation, but are not easily detected until the lid is everted. Occasionally there seems to be an attempt at a distinct row of lashes from the posterior edge of the lids.

The causes of trichiasis may be traced to any form of ophthalmia, or of inflammation of the lid, or of injury that alters the position and direction of the hair follicles, or, by keeping up an undue vascularity of the part, stimulates abnormal growth of cilia. In old cases of tinea ciliaris where small ulcers have formed round the roots of the lashes, and have healed irregularly, some of the lashes become displaced, and the direction of their growth changed and curved inwards. Wounds, burns, or even styes may produce it. The treatment of these cases is often attended with much difficulty; it may be palliative or radical. The former consists in simply drawing out the peccant lashes. In doing this care must be taken not to break it off at the root, otherwise the irritation may be increased. If the hair is very fine and pale, it is not only difficult to see it, but also to seize it. For this purpose a particular form of broad-pointed forceps, very carefully made, is required. In cases where there are only one or two hairs displaced, this is such a mild and simple proceeding, and there is so much difficulty in effecting a radical cure, that unless the patient is very desirous of having some attempt made, it is better to rest contented with the simple removal of the hairs. It may be repeated as often as they grow, and before they attain sufficient length and strength to keep up irritation. In more severe cases, where the inverted lashes are more numerous and of the usual length and strength, the palliative treatment is very unsatisfactory. Some are missed or broken off, and one or other is constantly creating irritation. Various methods have been suggested for accomplishing a radical cure in these cases, all of which occasionally fail, and none of which are suited to all cases. If there are only two or three lashes in a false position, and the symmetry of the parts is in other respects preserved, the manifest indication here is to destroy their bulbs and leave the other parts undisturbed. To accomplish this various suggestions have been made. Thus it has been recommended to remove the lashes and insert a fine cautery into the hair follicle; or, again, to insert a needle into the follicle, and then inoculate it with the tartrate of antimony so as to produce a pustule; or, again, it has been recommended to dissect out the bulbs. In practice these ingenious suggestions fail. When the hair is removed, no aperture can be discovered into which the cautery can be introduced. The inoculation is difficult, the bleeding usually prevents it from taking, and if it takes, the subsequent scar is liable to drag neighbouring lashes into the same predicament. Dissecting one or two lashes out is equally difficult, and equally liable to alter the growth of others. After trying various expedients in many cases, I am of opinion that it is better to rest content with the palliative method I have already described. If, however, the patient is bent upon having something tried, I think that the removal of a small triangular piece of the entire lid involving the part from which the inverted lash grows, is, on the whole, the most effectual and least objectionable plan. The divided parts can be accurately adjusted without any subsequent deformity.

In more severe cases, where there is a distinct extra partial row of lashes, either this plan must be adopted, or the case must be treated upon the same principle as entropium. Where there has been an irregular growth of lashes, the result of the healing of small ulcers or abscesses in their immediate neighbourhood, and where the inward tendency may be only just sufficient to permit the end of the hairs to touch the eye, a new cicatrix may be formed by removing a portion of the skin and subcutaneous tissue and muscle close to the root of the lashes, so that by the contraction of the cicatrix the direction of their growth may be changed. Modifications in doing this must be made to suit the specialities of each case, and no precise rules can be laid down. One remark only I would make as the result of my own experience. The displaced lashes have a tendency to relapse into their old state; therefore it is safer to remove rather too much than too little; and further, I think it will be admitted that, in the aggregate, these are unsatisfactory cases, particularly in the upper lid, unless treated upon the same principles as entropium.

We are naturally led on from this affection to entropium, or inversion of the eyelids. In these cases the entire row of lashes, and sometimes the lashes of both upper and lower lids, turn in and rub upon the surface of the eye, causing much distress, spasmodic closure, and in old and neglected cases, opacity of the cornea, more or less dense and vascular, and even amounting to a regular pannus, with consequent damage to the sight.

There are two forms of this disease that differ essentially from each other. In one, the orbicularis muscle acts an important part, and little or no organic change of the lid occurs; in the other, the inversion is rather due to organic change of form in the lid itself.

The first of these is almost entirely found in elderly people with loose superabundant folds of integument round the lids; it takes its origin in an attack of ophthalmia, and is limited to the lower lid. If under these circumstances the parts around and beneath the lid become swollen and infiltrated, and if at the same time the marginal fibres of the orbicularis muscle are thrown into undue and spasmodic action, the tarsal cartilage is made to perform a half revolution upon its axis, so that the upper margin, with its cilia, lie buried down in the palpebral fossa, quite out of sight; and the margin of the lid is formed by the loose skin rolled over the lower edge of the cartilage, which now occupies the position that the upper edge takes in its natural state. If the skin be gently drawn downwards with the finger, the lid resumes its natural state, and will remain so for a few seconds, when, after some rather sudden action of the orbicularis, it jumps back into its abnormal position. This is a rather common affection, and is frequently observed after the operation for the extraction of cataract. Owing to the loose state of the parts, and the complete way in which the lashes are turned downwards as well as inwards, the irritation is sometimes not very considerable; and it not unfrequently happens that the patient is quite unconscious of what has occurred, and this condition of ignorance is occasionally shared in by the medical attendant. Whenever it exists, it effectually prevents the parts from returning to a healthy state, and therefore means must be adopted to preserve the lid in its natural position.

The treatment may be divided into the temporary or palliative, and the permanent. The former consists in endeavouring mechanically to hold the lid in its natural position until the irritation, swelling, and muscular spasm shall have passed away. This may be done by means of a narrow strip of adhesive plaster. It must first be well fixed close to the margin of the lid near its outer angle, and then, having been drawn downwards to the necessary extent, the remainder must be fixed to the cheek. A more recent and ingenious plan has been recommended by Mr. Bowman, and consists in applying collodion to the entire surface of the lower lid, which must be held in its natural position, carefully dried, and painted over; this by its contraction keeps the lid out, but it requires to be renewed every two or three days. Except in mild and very temporary cases, this plan is not found to succeed so well in practice as *à priori* reasoning would lead us to expect. The constant flow of tears soon loosens either the plaster or the collodion; and the action of the orbicularis, ever at work to counteract these applications, soon succeeds in again inverting the lid. The only effectual cure for these cases is to pinch up and remove a small portion of loose skin close to the margin of the lid, and then to remove a corresponding portion of the orbicularis muscle. This is a very essential part of the operation, the importance of which was first strongly insisted upon by Mr. Haynes Walton. A little judgment is required to remove sufficient to ensure a normal position of the lid; this may be regulated with great nicety by means of a small pair of forceps with Assellini points, and a pair of scissors. The removal of a piece of the muscle has the twofold advantage of ensuring a subcutaneous contraction and of diminishing the power of that part of the muscle; it also admits of the removal of a much smaller piece of skin than would otherwise be required. The edges of the divided skin must then be carefully brought together with fine sutures. The subsequent scar is scarcely visible.

Another and a far more serious form of entropium results from a gradual thickening and contraction of the inner edge and surface of the tarsal cartilage, curling inwards, so that the lashes may be observed first to incline downwards, and then by degrees inwards: although this depends chiefly upon the altered form of the cartilage, it is increased and aggravated in its latter stages by spasm and hypertrophy of the orbicular muscle. This state of things may exist in either lid, but it is most commonly seen, and in its worst forms, in the upper lid. It is a morbid change of slow growth, and depends upon chronic ophthalmia, of the catarrhal type, extending to the fibro-cartilage, and producing thickening and contraction of this structure, together with a granular condition of the lining membrane. Not unfrequently, on everting the lid, traces of a cicatrix may be seen, which has probably been the main cause of the contraction, and may usually be traced to the application of some powerful caustic, as the nitrate of silver. The effect of this condition of parts is to keep up constant pain, irritation, and chronic inflammation of the conjunctiva, and to cover the cornea with a vascular membrane or pannus, thus seriously injuring the sight. The lids are usually kept spasmodically closed; the lachrymation is profuse, and the distress and suffering of the poor patients are very severe, and unless relieved by some artificial means, protracted and hopeless.

A great variety of plans have been suggested for the relief of this state of things, and much ingenuity has been expended upon it, but with only very partial success. When once parts have become altered in their form by a slow morbid process, it becomes nearly impossible to restore them to a normal state. One set of operators have chiefly aimed at a restoration of the curved and contracted cartilage; others have principally directed their attention to the alteration of the position of the eyelashes, or to their removal. The former object, if it can be accomplished, is the most scientific, and is the only one that really aims at the removal of the morbid state; the latter merely aims at the treatment of one of the most irritating results of the disease—the inverted cilia. Attempts have been made to alter the form and direction of the lid: thus vertical incisions have been made through the entire lid; sometimes through the centre, sometimes at the two angles, and the portions so divided have been everted, and fixed for a time in that position. But these are severe operations; they seriously mutilate and deform the lid, and their success in remedying the disease is doubtful and temporary. I believe it is nearly, if not quite, abandoned in the present day. The removal of the skin and muscle near the tarsal margin, however complete, seldom succeeds; the contraction and curvature of the tarsal cartilage is too inveterate to be counteracted by any such proceeding. As a further extension of this proceeding, it has recently been proposed to cut longitudinal grooves in the cartilage so as to diminish its inward curvature. My colleague, Mr. Streatfeild, has recommended a proceeding of this kind, and Dr. Snelling, of Utrecht, has suggested a very ingenious proceeding, which he has frequently put in practice, and of which he speaks most favourably. The plan Dr. Snelling adopts is somewhat as follows :—The entire lid is firmly enclosed in a wire frame similar to that suggested and employed by M. Desmarres, of Paris. The great advantage of this instrument is, that it entirely suspends the escape of blood, and thus facilitates the different steps of the operation. The skin is then raised from the surface of the cartilage, which is cleanly exposed. A longitudinal wedge-shaped piece of cartilage is then dissected out, leaving its mucous lining intact. The next step is to approximate the edges of the remaining portions of the cartilage with carefully-adjusted sutures, in such a way as to draw the lower edge, with the cilia attached to it, up to the upper, and at the same time to evert its lower border. The ends of the sutures are then fixed to the forehead with adhesive plaster, so as to avoid as much as possible any tendency to inversion. This is kept up for two or three days. The idea involved in this operation is excellent, the immediate effect is most satisfactory, and in the hands of Dr. Snelling the ultimate effects may be equally good ; but I feel bound to admit, that in the cases in which I have adopted it, I have found a strong tendency to relapse at the end of some months from the date of the operation, and I may add, that it is only after the lapse of many months that the advantages of any operation for this disease can be fairly estimated.

Other plans, as I have said, are directed to the removal of the chief cause of pain and mischief—viz., the inverted cilia. This may either be done by altering the direction of their growth, or by removing their bulbs altogether. Thus the lashes, together with their bulbs, have been raised from their attachment and transplanted, as it were, higher upon the surface of the lid ; but in all these attempts, however well they may succeed at the time, there is a strong tendency to relapse as the parts heal and as a cicatrix forms.

The plan that I usually adopt, and that is most commonly adopted at this hospital, is the one employed extensively by Mr. Wilde, of Dublin, and suggested long before his time—viz., the complete extirpation of the bulbs of the eyelashes. The operation is performed in the following manner :—The patient being placed under the influence of chloroform, a bone spatula, suitably shaped to fit the convexity of the eye and the concavity of the lid, is passed up between the two, and is held by an assistant in such a way as to raise the lid from the eye and to keep the lower edge well upon the stretch. An incision is now made along the entire edge of the cartilage, just within the roots of the eyelashes. This is carried back so as to raise the lashes with their bulbs from the surface of the tarsal cartilage. This requires a separation of the soft parts from the lid to the extent of about three lines. The next step is to remove with a pair of scissors all that portion of skin, including the bulbs of the lashes, that has been previously dissected up. The loose skin of the lid may then be allowed to fold over the wound and the operation is complete. Great care is required not to leave any of the bulbs behind, otherwise the object of the operation is partially frustrated. It very speedily heals, with only a fine line of cicatrix at the tarsal margin.

The objections to this operation are—first, that it partakes of the character of a mutilation; second, that it leaves one portion of the disease—viz., the curved, thickened, and contracted cartilage—untouched; third, that in spite of every care one or more bulbs are liable to be overlooked; and lastly, that it is rather a severe and painful proceeding except under chloroform.

On the other hand, it so entirely removes the chief source of irritation, the lashes, that the patient at once experiences great relief; the condition of the eye and of the sight rapidly improves, and the deformity is much less than might be supposed from a description of the operation.

After having frequently put the various methods that have been suggested to the test of experience, I am decidedly of opinion that in all severe cases the operation I have just described is the best, and for some time past I have rarely employed any other. Nevertheless there is yet scope for ingenuity to devise something more perfect, in which the lashes may be preserved, and the shape of the cartilage restored to its normal state.—*Lancet, Nov.* 29, 1862, *p.* 584.

76.—ON GONORRHŒAL OPHTHALMIA.

By Dr. M. H. Collis, Surgeon to the Meath Hospital and County Dublin Infirmary.

This affection—so formidable to the surgeon to deal with, and so fatal to the usefulness of the eye—yields with marvellous rapidity to repeated weak injections. The inflamed and œdematous conjunctiva being punctured, or snipped with the scissors if necessary, a careful student can be put beside the patient's bed, and shown how to send the contents of the syringe underneath the upper lid, from the external canthus across the eyeball. In the most acute cases a solution of a quarter of a grain of nitrate of silver to the ounce of distilled water should be used every ten minutes, for the first hour; after that a half-grain solution should be injected every half hour. If this is carefully carried out for the first twenty-four hours, the patient's eye will be quite safe. A stronger solution may then be used; and, if needful, it may be followed, in a couple of days by Guthrie's ointment of nitrate of silver, if the villous condition of the conjunctiva should seem to require it. I have followed this plan of treatment generally, for at least nine years; and in that time I have never lost an eye from gonorrhœal ophthalmia, with one exception; in that case the pupil in charge broke the syringe, and thinking it a matter of no importance, he waited for twenty-four hours to get it replaced; by this time the cornea had sloughed in one point, and the iris protruded. The man, however, was so fortunate as to recover, with comparatively slight injury to sight. Such surgeons and pupils as followed any of these cases have been struck with astonishment at the facility with which this formidable affection is cured. I cannot at this moment remember to whom the credit of weak injections of nitrate of silver is due; my attention was drawn to it by seeing the failure of the heroic treatment, which sacrifices nearly 50 per cent. of eyes in whole or in part. I found, however, that the weak solutions were insufficient for the cure of the disease unless frequently applied.—*Dublin Quarterly Journal, Feb.* 1863, *p.* 5.

MIDWIFERY,

AND THE DISEASES OF WOMEN, ETC.

77.—CASE OF INDUCTION OF LABOUR AT THE EIGHTH MONTH, BY DR. BARNES' METHOD.

[Eliza J., aged 30, was an out-patient at first, at the London Hospital, on account of incessant vomiting, which had persisted from the commencement of her pregnancy until now, the eighth month. Every remedy is useless, and she is extremely reduced in strength, all food being immediately rejected.]

Tactile examination reveals a scirrhus-like induration of the cervix uteri, which is nodulated. One very hard nodule, larger than a hazel-nut, occupies the right side of the anterior lip and cervix; there are several smaller ones. The catamenia ceased on the first of May last; and as she was so reduced by the vomiting, Dr. Barnes, at her own earnest request, appointed Christmas-eve, at seven P.M., to induce premature labour, remarking at the time that it would not interfere with Christmas-day, as he could promise to terminate the labour in less than eight hours. It was all over in half that time, as the sequel will show.

At fifteen minutes past seven on that evening Dr. Barnes introduced a small caoutchouc dilator into the os uteri by means of the uterine sound. It would then barely admit the tip of the little finger. In half an hour, by water pressure, two fingers could be admitted, and the vagina was nicely dilated. The stop-cock was then turned off, and the instrument removed for a little while. A rather larger dilator being then introduced, another half hour had enlarged the os uteri to admit nearly three fingers, but dilatation went on very slowly in consequence of the scirrhus-like induration. Dr. Barnes then ruptured the membranes at half-past eight P.M., and incised the os uteri at several points with a guarded penknife. The morbid thickening and rigidity of the cervix were extreme, and would have been incompatible with the birth of a living child at term. Some pains followed, and the dilator was again introduced, and pretty fully distended with water. After about half an hour more the dilator was removed, and opportunity given to allow the induced uterine action to work. A little before ten P.M., the os uteri was sufficiently dilated to allow Dr. Barnes to adapt the long forceps, with which, in another half-hour, he terminated the labour. The face was directed anteriorly. The placenta followed in a few minutes, and the uterus contracted well. The child, a boy of good size for eight months, was born semi-asphyxiated; but after half an hour's use of warm and cold water affusions, and Dr. Sylvester's method of artificial respiration, it respired freely. The mother has not had a single bad symptom, and the child, though feeble, bids fair to live. Dr. Barnes hopes that besides relieving the vomiting, (which it has done,) this premature delivery may, by the mammary diversion keeping the uterine and ovarian organs in quiescence, afford opportunity for recruiting the mother's general health.—*Lancet, Jan.* 3, 1863, *p.* 10.

87.—ON A CASE OF INDUCTION OF PREMATURE LABOUR BY DR. BARNES' METHOD.

By F. M. Corner, Esq., Surgeon to the Poplar Hospital.

The following case illustrates in a most satisfactory manner the perfection of the method of inducing premature labour introduced by Dr. Barnes, and is an instance of a most useful application of the method in a case which could hardly have been relieved so successfully and so speedily by any other means.

My patient, aged about thirty, menstruated naturally at the end of July, when she considered she became pregnant of her third child. From this date to Sept. 10th she had a slight discharge of blood, lasting a day or so and disappearing for several days; and then (Sept. 10th) a large quantity of aqueous discharge, with reduction of the size of the abdomen, but no change in the fulness of the breasts or other symptoms of pregnancy. On the 23rd of October a larger loss of blood took place, attended by shrinking of the mammæ, coldness in the abdomen and thighs, and a drain from the uterus of black thick fluid. Her general health underwent change, she feeling sickly, faint, and thoroughly "out of sorts."

I first saw her in the early part of November, and found a tumour, shaped like the pregnant uterus, reaching to the umbilicus, soft and doughy to the touch, and giving the impression of nothing beyond fluid within. Internally the os was found patent, the sound being readily passed several inches. After watching her for several days, I determined on exploring the interior of the uterus, believing in the presence of a diseased fœtus.

On the 8th of November, having opened the os somewhat by a sponge tent, I at a quarter-past eight P.M. introduced Dr. Barnes' second-sized bag, and inflated it, to secure moderate pressure within the canal, and left for an hour. It was then found that regular uterine pains had been established, and the os had dilated and seemed very dilatable. The largest-sized dilator was then passed, and at intervals of a few minutes was progressively filled, effecting like the one first used, dilatation and uterine action. At eleven P.M. the os was sufficiently dilated to allow of a full examination, when I found a tough membranous bag presenting, pressed down by each pain. This failing to make much progress, I gave a dose of ergot and punctured, liquor amnii escaping, and almost directly following, a fœtus, of, I should fancy, three months' gestation. The head was encircled by the os for some minutes, so that before expulsion I could not determine whether it was living or not; it was not at all decomposed. I left at midnight, congratulating myself on the issue of the case.

I may briefly mention what struck me particularly in the use of these instruments: the ease with which they were introduced within the os by the aid of a pair of long, straight, narrow-bladed forceps, commonly used, I believe, for cleansing or making applications to the uterus; the absence of complaint on the part of the patient; and their speedy action in dilating the parts and inducing natural expulsive pains. For the purpose intended by Dr. Barnes I have no doubt they will prove everything he has stated. Certain, safe, and speedy, what more can be desired? I never had any case the result of which pleased me more. Everything went on as one wished, neither more nor less.—*Lancet, Jan.* 31, 1863, *p.* 115.

79.—CYSTIC TUMOUR OF THE VAGINA.

Case under the care of Dr. BARNES, at the London Hospital.

[The patient was aged thirty-one, and had had four children. She had been aware of a swelling in the vagina for about four years.]

About a year ago, the tumour, becoming troublesome from its bulk, was punctured; some fluid escaped. It quickly filled again, and became larger than before. She was seen by Dr. Barnes in September. She was then four months pregnant. There was then a tumour seated in the anterior wall of the vagina, a little below the os uteri; it was of the size and form of a hen's egg, smooth, tense, elastic, giving the sensation of a hernia of intestine. By speculum, the tumour was seen to be covered by mucous membrane in a state of inflammation. The cervix uteri and the vagina, generally, were inflamed, secreting muco-pus, and in spots abraded, from the irritation caused by the friction of the tumour, which filled the vagina as a foreign body. It was concluded that the tumour was a cyst containing fluid, seated behind the mucous coat, and between the muscular and fibrous coats of the vagina. Its position was considered to exclude the conjecture that it might be a cyst formed in the broad ligament. Regarding the proximity of the tumour to the bladder, Dr. Barnes did not think it advisable to attempt to enucleate it entire. He, therefore, adopted a practice which he has found effective in all the varieties of vaginal cystic tumours. The patient being placed on her back, and the vagina opened by the duck-bill speculum on either side, the cyst was seized by the vulsellum, and punctured, when more than an ounce of clear, glairy fluid escaped, and an elliptical piece of cyst-wall cut out with the scissors. The remains of the cavity were then plugged with lint, soaked in tincture of iodine. Successive examinations showed the gradual shrivelling of the remains of the cyst-wall. For about three weeks there remained a small cup-shaped cavity, with an indurated base, the surface granulating. When discharged, at the end of a month, there was only a trace of the cyst left, the inflammation was entirely removed, and the patient was free from inconvenience. Independently of the distress caused by such a tumour in the non-pregnant state, it might, if existing during labour, give rise to a pelvic inflammation through the contusion caused by the passage of the head. There was, therefore, a double inducement to remove it. —*Med. Times and Gazette, Dec.* 6, 1862, *p.* 602.

80.—ON ERGOT OF RYE AND THE NATURE OF ITS ACTION ON THE UTERUS.

By Dr. Graily Hewitt, Lecturer on Midwifery at St. Mary's Hospital Medical School.

It may be taken as an undoubted fact, that the ergot of rye is very frequently found to produce a marked effect in exciting contractions of the uterus. That it not uncommonly is entirely useless when administered for the purpose of exciting uterine action, must also be admitted to be a fact. These two propositions imply either an absolute uncertainty in the action of the drug, or the presence of a peculiar idiosyncrasy in particular individuals, one set of patients being liable to be affected with ergot, and another not so liable. The only other alternative is to suppose that there is a want of potency in the particular specimens used; but the answer to this is, that particular specimens under the same conditions as regards freshness, physical condition, and otherwise have been found to vary in their effects as above described. To adopt the second alternative,—that particular individuals have special idiosyncrasies as regards the action of ergot, is hardly a satisfactory mode of explaining the matter.

On what, then, depends the fact that ergot is at one time active, and at another time without apparent action ?

To answer this question, we must first endeavour to ascertain what is the nature of the action of ergot in inducing contraction of the uterine muscular tissue, or in lessening the bulk of the organ. This question is one of the greatest possible interest and importance; but it is one which has not yet received a satisfactory answer.

Some little time since, while engaged in studying the subject of puerperal fever, and in tracing the connexion—a connexion the existence of which appears to be substantiated in a certain number of cases—between defective contraction of the uterus after delivery and the occurrence of this disease, I was struck with the fact that in so many cases ipecacuanha had been reported as having been exceedingly efficacious in preventing the outbreak of the disease, or in checking its progress at the early stage; while, as I knew from other sources, the same drug was also found to possess great efficacy in restraining uterine hemorrhage. It immediately occurred to me that this action of ipecacuanha in restraining uterine hemorrhage, and in preventing puerperal fever (and which was, doubtless, in both cases due to its action in exciting contraction of the uterus), must be connected with the vomiting excited when the drug was taken into the stomach. Thus far, then, the argument was: ipecacuanha produces contraction of the uterus, and ipecacuanha produces vomiting; probably, therefore, the contraction of the uterus is dependent wholly or chiefly on the vomiting. Considering next the action of ergot in producing contraction of the uterus, and being aware how frequently ergot, when given, sets up vomiting or occasions nausea, I was led to adopt the conclusion which it is now my object to enunciate; that the action of ergot, as an excitor of uterine contraction, is dependent on a like cause—viz., the vomiting and nausea often set up by its administration.

Such were the steps by which I was induced to adopt a theory as to the action of ergot which I have since found to be borne out by the facts which have come under my own observation. The theory is one, the truth or untruth of which can be easily determined by a sufficiently large number of well-observed facts. I regret that the observations at this moment in my possession are insufficient in number to give them any great scientific value in the determination of this question, although they appear to myself to be sufficiently convincing to warrant this my expression of belief on the matter.

The facts necessary to be observed are: 1. Ergot being given in a particular case, and a marked ergotic effect being produced, is this effect coexistent, or not, with vomiting or nausea? 2. Ergot being given, and no such marked ergotic effect induced, is there an absence of vomiting and nausea? (It would be advantageous to have a number of observations under this head). 3. Ipecacuanha or any other emetic being given, are the contractions of the uterus intensified thereby, or increased in frequency? Considerable care would be necessary in recording the answers to these questions, to give the observations a true scientific value.

Some very important results would follow the adoption of the theory now contended for in obstetric practice, and the question which was put at the commencement of this paper,—"On what is the uncertainty of the action of ergot dependent?"—would receive, in all probability, a satisfactory answer. I am quite aware that, even admitting that the above explanation is so far a true one, this would only be an advance on the road to a perfect explanation of the matter: the whole relation of vomiting to uterine action and activity will have still to be considered. It is well-known that uterine action itself very frequently excites vomiting; and it may be said, therefore, that, admitting that ergot acts only when it excites vomiting, this vomiting is not so much the effect of the ergot as an evidence of uterine activity having been excited. The physiological and the clinical elements in the consideration may, however, be very usefully separated. Clinically, it may be, as I believe, will be, substantiated, that ergot given without producing vomiting will generally be found useless as a parturifacient. What the explanation of the connexion between the vomiting and the uterine action may be is another question, which must be discussed separately.—*Lancet, Jan.* 17, 1863, *p.* 59.

81.—ON THE ACTION OF ERGOT OF RYE ON THE UTERUS.

By Dr. Wm. Seth Gill, Pentonville.

In my observations respecting the ergot of rye, and its specific action on the muscular fibres of the uterus—published in the Lancet some twenty years ago, on its first introduction to the profession,—I was led to offer some practical deductions, which past and present experience fully justifies.

The opinions expressed by Dr. Graily Hewitt caused me to make some investigation into his statements. His theory is not novel, as it is well known that antimony and ipecacuanha are the usual companions of the obstetrician; but when a statement is made so widely differing from the characteristic and known effects taught by daily experience—namely, that "the action of the drug is uncertain, and that it depends on its emetic power alone,"—I, for one, beg most respectfully to differ from him.

Having had a long experience in the use of this invaluable agent, I feel it incumbent on me to speak out, strengthened as I am by a vast number of cases, some of recent date, in which I have administered the ergot without producing even *nausea*, and *most assuredly without sickness.* I could not conscientiously be taciturn; nor will I assert that I have *never* seen vomiting produced after its administration; but I could not take upon myself to say that it arose from the effects of the ergot.

I am convinced, as far as my own experience leads me, that the profession have a valuable therapeutic agent in this drug, and that its effects have a *specific action* on the uterine muscular tissue, as certain as mercury on the liver. In what manner this takes place is a question I leave to physiology and science to disclose, satisfied, as I am, that it is a *fact*, recorded by the test of twenty years.

Dr. Hewitt does not tell us in what form he administers the drug. I have found the most efficacious to be, an infusion of from half a drachm to a drachm of the powder, and repeated, if necessary, in twenty minutes, with the addition of one teaspoonful of brandy-and-sugar. In this way I have seldom seen even the approach to sickness; but have ALWAYS been rewarded by a *safe* and *speedy* termination of my patient's sufferings.—*Lancet, Feb.* 7, 1863, *p.* 160.

82.—ON TARTRATE OF ANTIMONY AS AN OXYTOXIC.

By Dr. PARKER, of Nova Scotia.

At a Meeting of the Obstetrical Society of Edinburgh, Dr. Parker contributed the following notes:—Tartarized antimony has long been, as you are aware, in use in obstetric practice. Lecturers and authors have informed us, and still continue to do so, that the result of its administration is to advance the labour,—1st. By overcoming muscular rigidity; 2nd. By causing a free flow of mucus, and thus lubricating the vaginal mucous membrane. While not confining its use to primiparæ, they speak of it as more especially serviceable in this class of cases.

With this much theoretic knowledge of its action I, sixteen years since, commenced testing it practically, and it was not long before I observed that, in addition to the actions previously attributed to it, a very important end was effected in a large majority of cases by its administration. I refer to its influence on the uterine contractions; in other words, to this agent as a powerful stimulant to the involuntary muscular action of that organ. While relaxing the circular or sphincter fibres of the os uteri, I observed that it produced more powerful propulsive efforts, by stimulating the longitudinal and other muscular fibres concerned in effecting delivery.

As early as the years 1846 or 1847 I called the attention of some of my medical friends to the facts, and requested them to keep a record of cases in which this auxiliary agent was prescribed.

It is unnecessary that I should occupy time by detailing minutely reports from my case-book. A brief summary of what I have observed in reference to this matter in a very large number of instances, and the deductions drawn therefrom will suffice, and equally subserve the end I have in view.

1st. Tartarized antimony relaxes both voluntary and involuntary muscular fibre concerned in resisting and retarding labour; in other words, it overcomes the rigidity of the os uteri and perineal muscles.

2nd. By increasing the vaginal mucous flow, and lubricating that surface, it essentially aids the progress of parturition.

3rd. *It stimulates to increased contractile action the oblique, longitudinal, and other muscular fibres concerned in expelling the child.* It may be said that this increased contractile action is only apparent, and that the advance of the labour is due to the decreased rigidity and resistance of the os and more external parts, in consequence of the tartarized antimony having produced the results stated in the two preceding propositions. This objection is at once removed and proved to be incorrect by the the results following its administration, in cases not primiparous, where there is "inertia uteri" with the os fully dilated, the vagina patent, and the perineum not rigid. In just such labours I have been in the habit of prescribing the tartarized antimony instead of ergot, with the most marked results.

4th. Unlike ergot of rye it does not produce continuous contractions, but, in the majority of cases, enhances the power and force of the regular pains, and admits an interval of ease.

5th. It continues to some extent to exert its contractile influence after the expulsion of the child, and hence reduces the risk of hemorrhage. Of course this result will be modified by the interval that has elapsed between the administration of the antimony and the delivery.

6th. Unlike ergot, it does not interfere with the extraction of the placenta, by producing irregular uterine contractions. The superior and propelling portion of the organ, it is true, is generally more firmly contracted than natural after delivery; but the sphincter (the os) is mobile and yielding, and but little difficulty (no morbid condition of the parts being present) is experienced in this particular; abdominal pressure, and traction in the proper axis will almost invariably effect the end without injury to the cord.

7th. While perhaps more applicable to primiparæ, tartarized antimony may be administered with like results in subsequent labours and that at any age or stage of labour, without risk of injury to the uterine texture.

8th. Administered as I shall presently state, its emetic effects are not troublesome; not more so when they do occur than from ergot, when this latter, as it very frequently does, produces vomiting. This effect of the antimony, when present, will promptly yield to cold oatmeal, coffee, a mustard cataplasm over the stomach, or a dose or two of the trisnitrate of bismuth.

9th. I have never known depression of the vital powers, or more than temporary inconvenience, to follow its use when administered in the mode hereinafter mentioned; although I can readily conceive that there are cases in which it would not be admissible. In the selection of appropriate cases and discriminating in this matter, medical men must have recourse to, and be guided by, their experience and general principles.

10th. Tartarized antimony, by facilitating and shortening the process of parturition, as above mentioned, without risk to the uterine apparatus or general system, will, we may confidently expect, by conserving the vital powers and reducing the nervous shock, show, on more extensive trial, favourable statistical results in the mortality attending obstetric practice.

The foregoing observations and conclusions have not been arrived at hurriedly, and they are not deductions drawn from a limited number of cases, or, as you are aware, from a limited field of observation. While they will be found correct in a large majority of cases, I am free to admit that tartar emetic will not in all cases produce the same results. And, indeed, of what therapeutic agent can we write in other language? Once only (six or seven years since) I have met with a single recorded remark that would lead me to infer that the same *contractile influence* to which I have referred has been noticed by others. I cannot now lay my hand on the periodical which contains it, but I recollect the writer states that, after injecting two grains (I think) of tartarized antimony into the rectum in a case of tedious labour, for the purpose of overcoming muscular rigidity, *the pains very shortly became stronger and more effectual, or words to that effect.*

The rationale of its action is undoubtedly to be referred to the influence tartarized antimony exerts on the sympathetic system of nerves. The uterus, like the stomach and intestinal canal, is composed of involuntary or unstriated muscular fibre, and is, like the other abdominal organs, largely dependent for nervous supply on the sympathetic system. Now, we all know as the action of antimony on these hollow abdominal muscles or canals, that, when administered in sufficient quantity, forcible contractions of the circular and longitudinal fibres composing them, and relaxation of the sphincters, take place; just what I have remarked and recorded as occurring in the uterus. Similarity of texture and nervous supply would then analogically point to that which experience and observation has taught me is in reality the case in reference to the action of this agent in the uterus in the act of parturition.

Ergot of rye, while acting primarily and with more energy on uterine muscular fibre, exerts at the same time a secondary or inferior influence on the stomach, often producing, as I have before remarked, troublesome vomiting.

Now, the deduction one would naturally draw from the above observations is, that all, or most of what are termed "specific emetics," would, through the agency of the sympthetic system, exert their contractile influence on the uterus, and *vice versa*, as just illustrated in the case of ergot,—*i.e.* therapeutic agents which cause uterine contractions will have a similar action on the stomach and intestinal canal. I have not administered ipecacuanha with the same object in veiw, but have very little doubt that if tried it will be found to produce like results. Its taste and bulk would, however, other things being equal, give a preference to its more convenient ally, tartar emetic; while on the other hand, its depressing effects would not be so marked in cases where a continuance of the medicine should be considered advisable. In this connexion I may remark that it is suggestive, and at the same sime corroborative of the views here stated, that nausea and actual vomiting, occurring naturally during labour, very generally advance, and often materially shorten the process, by increasing the contractile power of the uterus. I administer the tartarized antimony as follows:—From one to two grains are dissolved in a wineglassful of water, and a dessertspoonful given every ten or fifteen minutes *ad nauseam;* and if the pains are not increased sufficiently, and there are no contra-indicating circumstances present, it is continued beyond this point. The rectal mode, to which reference has been made, has its advantages, and, at the same time, its inconveniences; but there are cases in which it should have the preference.—*Edinburgh Medical Journal, Jan.* 1863, *p.* 672.

83.—A SUDDENLY FATAL CASE AFTER LABOUR.

By T. L. Pridham, Esq., Bideford.

On October 2nd, I was called to attend E. T., of florid complexion, aged twenty-four. She was in labour for her first child, which was illegitimate. Her pains had been entirely confined to the abdomen from the commencement of her labour, and were described as "very cutting." She had been in labour about ten hours; it was about midnight when I first saw the patient. She had great apprehensions as to the safety of her delivery, more particularly as her mother had always severe times, accompanied, as she said, with convulsions. I had not been in the room more than a quarter of an hour before a pain came on in the lower part of the abdomen, which was quickly followed by the first convulsion, which lasted about a quarter of an hour, passing off with foaming at the mouth; and during the convulsion her tongue had been severely bitten. The pulse was then at 120 in a minute, free and bounding. The pupils were much dilated. Before I had time to tie up the arm, in order to bleed my patient, another pain came on, which was immediately followed by a convulsion not less severe than the first, though not of so long duration. Thirty-five ounces of blood quickly flowed, by which a decided impression was made on the system, as the pulse fell considerably, and she became faint and pale. Her bowels had acted freely during the day, and she had passed her urine just before I arrived. I lost no time in examining the stage of the labour, and found the head presenting above the brim of the pelvis, the os uteri three parts dilated, and the membranes entire. I at once ruptured the membranes, and proceeded to turn the child; but in the attempt I encountered an almost insurmountable barrier in consequence of the irregular action of the muscles of the uterus, which, about its centre, embraced the body of the child so firmly that I was obliged gradually, and with much difficulty, to introduce my fingers, and lastly my hand between the child and the band I have already described, to reach the upper chamber, as it were, of the uterus, in order to get hold of a foot, the contraction still acting most powerfully on my arm. The convulsions, which now occurred about every twenty minutes, had become less severe and of shorter duration since the abstraction of blood. They were still preceded by pains in the abdomen, during which time I, of course, relaxed in my efforts to turn. I should here remark, that perfect consciousness did not return after the first convulsion; although, when the patient was addressed by me in a very decided manner, she would give a confused answer to the question put.

I was much struck with the remarkable expression of the countenance of the patient whilst in pain, which gave, instead of the evidence of suffering, a kind of idiotic smile. My efforts to turn and deliver were fruitless, as the contraction still continued as firm as ever; the convulsions, however, gradually subsided, and by three in the morning had quite left her, as well as the abdominal pain. At this time, I made another effort to turn, and got down both feet; but the evolution of the child could not be effected, as the irregular action of the muscles of the uterus had not given way, and the body of the child remained perfectly fixed in its original position.

I now gave a full dose of laudanum hoping that its effects would act beneficially in relaxing the unnatural contraction of the muscles of the uterus, which the bleeding had failed to do; and having, with difficulty, replaced the feet in the upper chamber of the uterus, I waited for two hours, during which the patient had some sleep, which was interrupted by occasional slight pains.

At this juncture, feeling my apprehensions increasing as regarded the safety of my patient, I requested the assistance and advice of Dr. Felce, a medical friend who happened to be in the neighbourhood. Having made a careful examination, he was of opinion that all efforts at turning would be fruitless. Under existing circumstances, it was deemed advisable to repeat the dose of laudanum, and wait. Up to this time, the pulse was favourable, and the patient had taken nourishment; there were short intervals of sleep. It now became evident that pains in the back existed, as she called for support in that direction; this gave us great hopes that all would end well.

Three hours afterwards, I made another examination, being resolved, as the powers began to fail, to deliver by some means or other; and, to my great satisfaction, I found that the head had descended into the hollow of the sacrum. As the efforts were becoming very feeble, I at once delivered by means of the forceps, under the influence of chloroform. It was evident that the child had been dead some days. The uterus contracted well, and no hemorrhage followed; she took in food; and her consciousness in a great measure returned; and so far we were thankful to feel that our anxieties in a great measure were over.

Shortly after her delivery, a sudden change appeared to come over her countenance; and, without further warning, life was quickly extinct. I should state that the blood taken from the arm was greatly buffed and cupped.

It is worthy of notice, that the state of the atmosphere during the time of labour was most oppressive, and lightning and thunder occurred at intervals during the night. I allude to these circumstances, as men of note, such as Denman and Smellie, have spoken of the probable influence of a great amount of electric fluid in the atmosphere in attacks of puerperal convulsions; then, again, Ramsbotham, in his able work on Obstetric Medicine and Surgery, says, in speaking of the remote causes of convulsions in childbirth (page 451); that, amongst other causes, he attributes those of the death of the child and the depressing passions; but the most frequent is some deranged state of the uterus itself,—probably, the nervous system—and consists in some irritation propagated from that organ to the brain. I should mention that the poor woman had become greatly dejected by being informed that the father of her child was a married man, to whom she was to have been united on the very day this unexpected circumstance was communicated to her. It is most probable that all the combined circumstances which I have related acted on the sensorium, resulting in the fearful convulsions which so often are the forerunner of death in childbed, as there was no paralysis or evidence of lesion of the brain whatever. I can only regret that it was not in my power to procure a *post mortem* examination, in order to prove that puerperal convulsions may be produced from exciting causes remote from the brain.—*British Med. Journal, Dec.* 13, 1863, *p.* 608.

84.—ON THE EMPLOYMENT OF THE FORCEPS.

By Dr. S. Laurence, Montrose.

[The following is part of a paper reviewing a thousand midwifery cases which have occurred in the writer's practice. It is only through statistics such as these that our pathway to truth lies.]

The number of cases terminated by the aid of the forceps was 28. Of these 21 were primiparous, and 7 non-primiparous, giving thus the following numerical proportions :—

Forceps cases	28 to 1000 or 1 to 35·7
Primiparous do.	21 to 220 or 1 to 10·47
Non-Primiparous do.	7 to 780 or 1 to 111·4

There is thus brought out the instructive fact that the forceps were required more than eleven times more frequently in first than in subsequent cases.

Results to Mothers.—All the mothers in these 28 cases recovered, except one, in whom the labour from contracted pelvis was peculiarly severe. Uterine phlebitis occurred a day or two after delivery, and ran a short, and, as usual, fatal course. It was the patient's second labour. Her first, owing to the same cause, had also to be terminated by the forceps. In some of the cases, where the labour had been considerably protracted before the forceps was employed, recovery was slow; in the great majority, however, it was as rapid, and, in all respects, as satisfactory as in ordinary cases.

Results to Children.—Four of the children were born dead, twenty-four alive. In regard to three of the dead-born children I feel quite persuaded that an earlier use of the forceps would have saved them. I give their particulars in a few words. One of them was my own patient from the commencement, the other three were, in the first place, under the charge of midwives. No. 1 (my own case). Primiparous—labour tedious—head for eight or ten hours low down in the pelvis, and making no progress—strength and spirits getting exhausted—put under chloroform, forceps applied easily, and delivered in a few minutes. Child dead. At the time this case occurred I was less impressed than I now am as to the danger (especially in *first* cases) of such delay as is here stated, otherwise I should have used the forceps some hours earlier, and in all probability have delivered a living child.—No. 2. Non-primiparous. Midwife had been previously fourteen hours in attendance. Uterine action teazing and inefficient—head well down for hours—patient's strength getting exhausted. Forceps applied at once, and child delivered, but dead. Recovery tedious.—No. 3. Called to this case after a very lengthened attendance by midwife. Head well down, but had for hours been making no progress. Applied forceps, and delivered. Child dead. Mother's recovery very protracted.—No. 4. In this case the child perished from convulsons, and from the position of the head, when I was called to assist, I do not think it would have been practicable to have saved the child by means of the forceps, even had I been earlier in attendance. Whether turning when the convulsions commenced, had I then had an opportunity of performing it, would have saved the life of the child, is, of course, aside from the point at present under consideration. My infantile mortality in forceps cases was thus 4 to 28, or 1 to 7; but, for the reason above stated, I consider this by no means fairly represents the safety and advantage with which that instrument may be used. So far, therefore, from agreeing with Dr. Murphy, "that so far as children are concerned (in protracted labour) the proportion of stillborn is very much the same whether the forceps be employed or not, the difference being in favour of leaving those cases to nature;" all my experience and reflection force upon me quite an opposite conclusion. Professor Simpson, in a discussion in the Edinburgh Obstetrical Society, reported in the Medical Journal for October 1862, remarked that "he had never doubted that the chief danger from delay (to the child) began when the fœtal head begun to be compressed in the maternal canals; but it was quite impossible to bring this out in any statistical table, as data enough were not to be had." There is, however, it seems to me, one way in which such a proposition admits of being invested with almost the force of a demonstration. It is to compare the practice of those who hold such a view with that of those who either reject or disregard it. Different beliefs necessarily prompt to different modes of practice, and the truth or error of the former becomes consequently embodied in the latter.—"By their fruits ye shall know them." Not to enter into any elaborate and extended comparison, just let us take, on the one side, Dr. Hamilton of Falkirk, who most earnestly believes this proposition, and on the other, the Dublin Hospital, where it is disbelieved or disregarded. I take the latter as represented in the report of Drs. Hardy and M'Clintock (1848). Dr. Hamilton tells us of his 90 to 100 forceps cases out of 731 labours, being 1 to every 7 or 8 cases. Drs. Hardy and M'Clintock relate 18 forceps cases out of 6634 cases of labour, being 1 to 368·5 cases. In other words, taking Dr. Hamilton's average as 1 to 8, we find he uses the forceps forty-six times for once they are used in the Dublin Hospital! Let us see the results of such contrasted modes of practice. In all Dr. H.'s 90 to 100 forceps cases the children were born alive. In the 18 forceps cases in the Dublin Hospital the mothers perished in 5 and the infants in 8! It will not do to throw the blame of this startling mortality, as is so readily done by the reporters, on the ergot of rye. Others use this drug as a uterine stimulant, without finding it possessed of such deadly potency as it seemed to exert across the channel. We must seek elsewhere for the cause of such disastrous results, and we shall only find it, I fear, in Dr. Murphy's principle having been acted on of "leaving to nature," for an unduly protracted period, case after case, which a timely use of the forceps would have conducted to a prosperous issue. My own experience is directly corroborative of what I have here advanced. During the first half of my 1000 cases I used the forceps only eight times, or once in 62·5 cases; during the latter 500 cases I used them twenty times, or once in every twenty-five cases; that is to say, just two and a half times more frequently in the latter than in the former. And what has been the result? The whole of the four fatal cases occurred during the former period! In other words, during a series of 500 cases, at a period when I considered the forceps only called for if the labour was considerably protracted, I used them eight times, and in these eight cases had four dead-born children;* while during another, a series of equal magnitude, at a period when I believed that in midwifery, if anywhere, "delays are dangerous," I used them twenty times, and in these 20 cases all the children were born alive! I leave this fact to teach its own lesson.

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* Strictly speaking, I should only count three dead-born children as due to the delayed application of the forceps, inasmuch as the death from puerperal convulsions

In the light of such facts, and others of kindred character constantly accumulating, it appears to me that there is no presumption in asserting that the teaching of many of our standard works on the use of the forceps requires revision. It would occupy too much space to cite passages by way of exhibiting the character of that teaching; suffice it to say, that in enumerating the circumstances which ought to prompt us to the use of this instrument, *danger to the child from protracted labour* is not dwelt upon with the force and emphasis it deserves. The perils which beset the mother from such a contingency are doubtless faithfully detailed; and hers, as being the more valuable life, ought ever to occupy the first place in our anxieties as practitioners of midwifery. To save our patient many weary hours of bodily pain and mental suffering, and so to place her in circumstances more propitious to a favourable recovery, would assuredly be a sufficient motive for the timely use of the forceps, even could no other be adduced. But the welfare of her progeny is also of urgent importance, and the measures needful to secure it demand our most serious thought. Be it observed, then, that often long before the development of symptoms menacing the life or welfare of the mother, the duration of the labour may seriously endanger the life of the child. A close investigation of the statistics bearing on this point will be found to fully establish the statement here made. There may be neither any notable exhaustion of the general powers of our patient, nor any such local heat, dryness, or swelling, as to indicate that the pressure of the fœtal head is compromising the safety of the maternal structures, and yet *the parturient process may be passing the limit in point of duration at which it is compatible with the birth of a living child.* It, therefore, is my thorough conviction, that danger to the child from protracted labour ought to enter more than it has ever yet done as an element into our calculations when deliberating on the propriety of using the forceps. The danger to the mother from the use of the forceps is often insisted on as an argument for their sparing employment. That much mischief may be

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was not owing to this cause. The general argument is but slightly affected by the admission. If it should be said that in two of the remaining three cases, the time when the forceps were applied, did not depend upon my abstract views of such cases, but upon the period at which my assistance was sought by the midwives in attendance, I have to reply that such personages, in their conduct in these matters, will be found to reflect the opinion of those whose help they solicit. How came it to pass that in all the after cases wherein midwives asked my assistance in their difficulty, the use of the forceps saved the child? Simply because the necessity of asking such assistance at an earlier period had been impressed upon them.

done by them, if rashly and unguardedly handled, is at once admitted. But I am not arguing for their careless, any more than for their indiscriminate employment. If intelligently and cautiously introduced and used, as they ought always to be, the risk of injury from their enployment will be found to be to a great extent imaginary. Every one who has had much experience in their employment will corroborate this averment.

While the general principle of the benefit accruing from the timely use of the forceps may be held as clearly established, the question is left open whether, in any given series of successful cases, they may not have been occasionally unnecessarily employed. Whether, in other words, a proportion of the cases in which they have been used, would not have been equally fortunate without them? Of course it would not be easy to answer this question, nor is it needful to do so; for, provided it can be shown that the practice pursued has resulted beneficially to the infants, without any present or after ill-consequences to the mothers, this is enough. By all means let the *nimia diligentia* be unsparingly censured, when it can be unequivocally proven. But we ought ever to remember that while to *cavil* against any given line of practice is an easy attainment, *to prove it wrong* is always a more difficult task. I should state that the forceps I have long used have been those of Dr. Ziegler of Edinburgh, the excellence of which is too well-known to need any commendation. I may further mention that in two of the 28 cases in which the fœtal head was well down in the pelvis, I made trial, in the first instance, of Dr. Simpson's "Air Tractor," (more euphoniously termed "Suctorial Tractor,") but found it quite unavailing for the purpose of extraction. The forceps had to be finally used in both. I have not learned whether the fortunes of this simple and *theoretically* happy contrivance have been at all more prosperous in the hands of others; or whether it has to any considerable extent superseded the use of the forceps, even in the practice of its ingenious inventor.—*Edinburgh Med. Journal, Feb.* 1863, *p.* 716.

85.—ON THE PREVENTION OF LACERATION IN FORCEPS CASES.

By Dr. Charles Ricketts, Birkenhead.

[There is no doubt but that there is a greater tendency to laceration of the perineum when the forceps are employed than in ordinary cases, and this somewhat militates against their use in cases in which they are really required, Dr. Ricketts observes,] The means which I have been in the habit of adopting in all forceps cases, for a long series of years, obviate, I think, all danger of lacerating the perineum, except to a very slight extent in primiparæ. When the forceps have been applied, and the head of the child has been brought down sufficiently low on the perineum that the forefinger of the accoucheur introduced into the rectum can reach, for instance, in occipito-anterior presentations, beyond the frontal eminence, the instrument should be withdrawn, or, if that be not convenient, all traction should be discontinued, and the finger, still retained in the rectum, should be used after the manner of the vectis, pressure being made with it, first on the space between the frontal eminences and the root of the nose, and afterwards on the upper jaw, and perhaps, if necessary, on the chin, as these parts successively come within reach. By this means the head is made to rotate in the axis of the outlet, and, being directed forwards under the pubes, its pressure on the perineum is greatly relieved.

I have adopted the above plan not only in a large number of forceps cases, but also in hundreds of instances of ordinary labour, with the effect of greatly expediting delivery during the last and most painful stage, and of protecting the perineum to an extent which the usual method of supporting it cannot accomplish. In no case have I found any inconvenience or injury to arise from the pressure of the finger upon the recto-vaginal septum.—*Med. Times and Gazette, April* 18, 1863, *p.* 415.

86.—ON A CASE OF RUPTURE OF THE UTERUS.

By Alfred Haviland, Esq., Surgeon to the Bridgwater Infirmary.

On the evening of the 5th of July, 1861, I was hastily summoned by an old practitioner to assist him in the delivery of a woman whose case presented some alarming symptoms. On my arrival, at about half-past eleven P.M., I found a tall, well-formed woman lying on her left side, flooding, vomiting, fainting, almost pulseless, with cold perspiration standing all over her face, and complaining of a fixed pain right across the umbilical region. Without delaying a moment to learn the history of the case, I passed my hand into the vagina, but could find no presentation at the os uteri, which was dilated sufficiently to admit my forefinger. After a little gentle perseverance, I got my hand into the womb, and was surprised to find it empty. On careful exploration, however, my fingers came in contact with the promontory of the sacrum. This revealed in a moment the nature of the case. I then separated as widely as possible my first and second fingers, and distinctly felt the edges of the laceration in the womb. Being now satisfied that the fœtus had escaped through the opening into the peritoneal cavity, I carefully proceeded upwards and backwards, and, for the first time in my life, felt the human kidneys, *in puris naturalibus*, warm and alive. The intestines surrounded my hand, and produced a most sickening sensation. On turning my hand forwards I immediately recognised the *unbroken* fœtal sac, which, with its contents seemed to float in the intestines, just under the stomach. So mobile, indeed, was this globular mass that I could easily move it round with my forefinger, and thus obtain any presentation I liked without difficulty. After rupturing the membranes, I seized the feet, which I found were small and disproportioned to what appeared a very bulky head. Having carefully brought the whole through the fissure, which extended obliquely through the posterior part of the uterus from the cervix to the fundus, I succeeded with little difficulty in getting the legs and shoulders into the world; but at this point an arrest of the child's progress took place, which revived my first impressions about the size of the head. However, by dint of patience and perseverance for half an hour, and strong pressure with my fingers on the skull, I managed so to modify its form as to enable it to pass through the straits; the placenta immediately followed, without hemorrhage. I then replaced my hand in the womb, assured myself that there were no folds of intestine in the fissure, gradually withdrew it, and found the uterus contract around it as it receded. Immediately afterwards I gave the patient a dose of ergot in brandy, and ordered it to be repeated at short intervals, in small doses, so as to ensure a permanent contraction. I need scarcely say that during the whole of the delivery I made the attendants give her raw brandy constantly, a teaspoonful at a time—a much more efficacious mode of administration than larger quantities at longer intervals.

After the delivery the woman seemed perfectly relieved; her pulse rallied, and the warmth of her skin returned, although the pain across her abdomen continued, and she occasionally vomited a dark-coloured fluid. Before leaving I prescribed a full dose of opium, after which she fell asleep. The child was a male, and, as well as I could judge from the misshapen mass the head presented after delivery, its circumference in utero could not have been less than eighteen inches.

As the patient lived several miles from Bridgwater, I was unable to watch the case; but on the Sunday following I visited her, and found her going on as favourably as possible under the circumstances. She had passed her urine freely ever since her delivery; she had suffered but little from after-pains; the pulse was regular, firm, but compressible, and 78; a warm moisture softened her skin. She, however, still complained of the abdominal pain, although its acuteness had subsided into an aching. I ordered her a dose of castor oil, which had the effect of relieving the bowels satisfactorily, as well as of stopping the sickness and nausea. I ultimately left her with some hope that her life might be spared; but a slight febrile attack supervened on the Tuesday following, for which, as I learned from her medical attendant, salines were substituted for beef-tea, brandy, and opium. She died on Wednesday, July 10th, six days after the rupture.—*Lancet, Feb.* 28, 1863, *p.* 232.

87.—ON THE MORE FREQUENT VARIETIES OF POST-PARTUM HEMORRHAGE.

By Keith N. Macdonald, Esq., Wrexham.

So far as my own experience is concerned, I have observed three different varieties of hemorrhage after delivery; one is when the hemorrhage either precedes or follows extraction of the placenta; another, when an interval elapses between the birth of the head and body of the child—a sudden and profuse gush follows extraction of the latter; and a third, when the hemorrhage for the most part takes place internally. The last two are by far the most dangerous. Should the hemorrhage be alarming in its character—if a short time intervenes between each gush, it is astonishing to what extent blood may in this way be lost without proving immediately fatal. But if, on the other hand, the gush is sudden and profuse, or the hemorrhage internal, syncope and collapse immediately follow. The most reliable symptom of internal hemorrhage is vomiting; next, restlessness, pallidness, and collapse. The occurrence and duration of the discharge are sufficiently diagnostic of the others.

Now, since the treatment of these alarming cases requires great confidence and coolness, it is not only necessary to be acquainted with the most efficient remedies, but to be prompt in their application. I do not see what benefit can accrue from having the head stuffed with a long list of remedies—such as ergot, acids, lead and opium, compression of the aorta, galvanism, &c., when pressure by the hands' grasp, cold, and the introduction of the hand into the uterus, are the most efficient and most easily procurable measures that can be adopted. Who would ever think of sitting down to write a prescription, to prepare an enema syringe, or send for assistance or a galvanic battery, while he could procure cold water and use both hands? I do not mean by this to disregard altogether the therapeutic uses of ergot and opium in their several modes of action; but to cases of post-partum hemorrhage I consider them wholly inapplicable, for the patient is either dead or the hemorrhage has ceased before they possibly could have passed through the system. I have had many cases of this kind, and have always found the above remedies (pressure and cold) to act promptly and efficiently.

Within the last three months I have met with two cases that have rather surprised me. One was that of a primipara, who suffered from hemorrhage after artificial delivery to such an extent that I positively declared that she could not survive the loss. The scene around us had more of the appearance of a slaughterhouse than a lying-in chamber; but my astonishment was almost unbounded to find her next day sitting up drinking a cup of tea. The other was a case of internal hemorrhage. On my arrival, the patient was vomiting, very restless, and talking incoherently; pulse imperceptible, and the uterus much distended. I was refused permission to remove the clots per vaginam; but seeing that death was imminent, I exerted strong pressure over the uterus, and, as it were, forced the coagula away in enormous masses.

The amount of stimulation necessary in these cases has, I believe, no limits. In two cases, at least, I have given as much as twelve ounces of brandy in four hours.

In conclusion, I may mention that these remarks are simply practical deductions from what has fallen under my own observation, apart from the more important and collateral pathological questions.—*Lancet, Dec.* 27, 1862, *p.* 698.

88.—ON VESICO-VAGINAL FISTULA, THE MODE OF OPERATING, AND THE RESULTS OBTAINED IN FIFTY-FIVE CASES AT THE LONDON SURGICAL HOME.

By I. Baker Brown, Esq., Senior Surgeon to the London Surgical Home.

[The following is an abstract of a paper read at a meeting of the Obstetrical Society of London.]

In this paper the author gave an account of the method at present followed by him in operating. The various steps of the same were illustrated by drawings. No bars or clamps are used. The knives employed are two—one for the right hand, and one for the left. The needles, of various curves, forming a series fourteen in number, are on the same principle as Startin's, but of rigid material. They are armed with wire, and thrust through the pared edges, great care being taken to avoid the mucous coat of the bladder. The two ends of the wire are simply twisted round and round, and so fastened. The patient is afterwards laid on the side, and a male elastic catheter, with a bag attached, kept in the bladder. She is kept quiet ten or fourteen days, and the wires removed. The operation is often completed in ten minutes.

The total number of cases of vesico-vaginal fistula admitted into the London Surgical Home since its foundation four years and a half ago is 58. Of that number, 55 were submitted to operation, with the results as shown in an accompanying table. The remaining 3 were not operated upon in consequence of the bad condition of bodily health, the result of syphilis. Of the 55 cases treated, 53 were operated upon by the author, 1 by Mr. Nunn, and 1 by Mr. Harper. Of the total number of operations, 43 were followed by perfect cure, 1 was much relieved, 2 died, 5 were not cured, and 4 are still under treatment, with every prospect of cure. Of the 43 cures, in 24 this result followed the first operation, including the cases of Mr. Nunn and Mr. Harper; in 8 the cure occurred after the second operation; in 5 after three operations; and in 6 after more than three operations. Of the other cases which were not cures details were given in tables exhibited. Of the two fatal cases, 1 died eighteen days after the operation, apparently from exhaustion, the age of the patient being fifty-six; the other died seven days after from pyæmia.

With regard to the causes of vesico-vaginal fistula. Of the 58 cases admitted into the London Surgical Home, 47 were over twenty-four hours in labour, and 39 were as much as thirty-six hours or more; 7 were two days, 16 were three days, 3 were four days, 2 were five days, 2 were six days, and 1 seven days.

In the whole number of cases, instruments were used in 29, exactly one-half; and in 4 only of these was the labour less than twenty-four hours, and with seven exceptions the patients had been thirty-six hours or more in labour before instruments were used.

Of the 58 cases, in 24 only the injury happened at the first labour, in 7 at the second, in 5 at the third, in 4 at the fourth, in 6 at the fifth, in 2 at the sixth, in 5 at the eighth, in 1 at the ninth, in 1 at the thirteenth, in 1 at the fifteenth, and 2 not mentioned. In many of these cases, notwithstanding the existence of the fistula, the patient bore several children, apparently without inconvenience, before coming under treatment; and in a few of them, subsequent to cure by operation, other children have been born without recurrence of mischief. In a large proportion of the cases there is a history of the birth of a very large child; in some it weighed 15 lbs.; and in one, that of the woman in whom the lesion happened at the fifteenth labour, the child weighed 17 lbs.

From the foregoing statistics it is evident that the cause of the lesion is protracted labour, and not the use of instruments or deformity of the pelvis; and, as a necessary conclusion to what has been stated, it follows that vesico-vaginal fistula would scarcely or never occur if a labour were not allowed to become protracted: this is a point for the careful consideration of the Society and of practitioners at large.

A printed tabulated statement as to the 55 cases operated on was handed round. Mr. Brown further stated that he had had 11 other cases under his care in St. Mary's Hospital, and 6 in private practice, making a total in his own experience of 58 cured; 34 by one operation, 11 by two operations, 5 by three, and 8 by more.

[In the discussion which followed]

Dr. Oldham said he saw nothing in the communication made to the Society, valuable as it was, to induce him to vary a hair's breadth from the precautionary precepts for the employment of instruments in protracted labour which were recognized and advocated by the best British obstetric authors. He felt persuaded, from his experience of cases which had come before him, that the more frequent employment of instruments would result in the laceration of the structures at the floor of the pelvis, particularly the laceration through the sphincter of the rectum, an accident only second to that of vesico-vaginal fistula in its distressing results, and which involved a most painful and formidable operation.

Dr. Tyler Smith observed that in laying down a rule of practice it was especially necessary to employ precise language. If Mr. Brown had employed the term labour with impaction, instead of protracted labour, he should have agreed with him. We might have protracted labour from many causes, some of them not necessarily injurious to the mother, but we could not have continued impaction of the head without the risk of injury to the soft parts of the mother, and impaction should therefore never be allowed to go on unrelieved. In performing the valuable operations which had been devised for these distressing conditions, he (Dr. Tyler Smith) thought it should be laid down as a principle, that in repairing one organ the integrity of the neighbouring organs should be respected. This applied, in the operations for vesico-vaginal fistula, to the rectum, ureters, and the os uteri. In some of the cases related the os uteri had been closed, and the menstrual fluid left to escape *viâ* the bladder, or the urine to pass per rectum. These evils were scarcely less than those they were intended to remedy. Fatal results had followed from closure of the os uteri. Very recently he had seen a remarkable case where the os uteri had been closed in a plastic operation, which seemed perfectly justifiable, for the relief of utero-vesical fistula. A perfect cure was the result as regards the urine for the time. Menstruation occurred through the bladder, but the patient in a few months became pregnant. The urethra was of moderate size, and there seemed no other explanation than that the spermatozoa must have passed from the vagina through the urethra and bladder to the uterus. The result of this and other cases satisfied him that it was not a safe practice to close the os uteri, or to invade other organs, but that the plastic operations should be limited as far as possible to the closure of the original fistulæ.

[Mr. Baker Brown, in reply, said that]

In America, Drs. Sims and Bozeman did nearly all the denuding of the edges with scissors. This made the operation very long. But if the knife was first used to mark around the fistula how much was to be taken off, and then, by means of a fine pair of forceps, the edges were made tense, Mr. Brown said that the whole fistula might be denuded, taking out a complete ring—a matter of great importance. In regard to the sutures, Mr. Brown stated that he greatly preferred wire, although Dr. Hayward, of Boston, U.S., used silk in preference. Mr. Brown had by this gentleman's advice used silk in one case with a good result. But they were more liable to slough, and the wires, from their stiffness, had the advantage of keeping the edges more in apposition, and so ensuring a greater depth of union. But the sutures to be used should, Mr. Brown thought, be silver, not iron, as recommended by Dr. Simpson, of Edinburgh. Mr. Brown expressed a very strong opinion in favour of keeping the sutures in long enough; never less than nine days. He believed no harm ever resulted from retaining them in longer; but in one case he had had, he removed them on the sixth day, at the patient's request. The fistula appeared quite healed. She got up next day, contrary to advice, and the whole burst open again. She was re-operated on; the sutures were left in a sufficient time, and a cure resulted. Mr. Brown had made experiments, and kept silver sutures in for six and nine months, and no trace of ulceration appeared. Mr. Brown considered that opium should be given only in sufficient quantities to keep the bowels quiet. One grain immediately after the operation, and repeated night and morning, according to the circumstances of the case. Many patients cannot bear the catheter, especially the leaden one, but Mr. Brown had seldom found the male elastic catheter to cause irritation. Perhaps in time we might gain sufficient confidence in the operation to allow the patient to pass her urine as required, but at present it was better to retain a catheter, or when that was not tolerated, to pass one when needful. He had purposely used the term protracted without reference to the cause of protraction, for it was very rarely that a medical statement could be obtained. It was necessary, therefore, to take only the statement of the patient as to the duration of labour. When the head was impacted, Mr. Brown would deliver as soon as possible by forceps. He quite agreed with Dr. Tyler Smith as to the necessity of maintaining the integrity of the other parts in these operations. Of the two cases on which he (Mr. Brown) had operated, and where the patients subsequently menstruated per urethram, in one, the patient having been under many operations in other hands for some years, the os had already been interfered with either by sloughing or by the operation, and only one small opening, as described, remained to be closed. There was, therefore, nothing left to do but to close the opening, regardless of the menstrual flow. The patient had done well. No inconvenience had arisen, and she was grateful for the benefit received. In the other, that lately brought before the Society by Mr. Chapman, the os uteri was already closed by cicatrization, after extensive sloughing, and therefore Mr. Brown had nothing to do with its occlusion. He considered that the two terms, vesico-vaginal and vesico-uterine fistula, were not used with sufficient caution. Although the os uteri might often be involved in vesico-vaginal fistula, the true vesico-uterine fistula was when the whole was high up in the cervix, urine dribbling through the os uteri, although no aperture from the bladder was visible. This was the kind of which Jobert had related seven cases, Dr. Fleetwood Churchill, one, and which had lately been so well described by Mr. James Lane, who had had one case. In all these the treatment had been to close the os, so that the patient menstruated per urethram. He (Mr. Brown) had never had such a case, but he thought that the treatment adopted was decidedly the lesser evil, and if the patient, on having the case laid before her, was of the same opinion, he considered it perfectly justifiable.—*Lancet, March* 28, 1863, *p.* 358.

89.—ON VESICO-UTERINE FISTULA.

By James R. Lane, Esq., Surgeon to St. Mary's Hospital.

A vesico-uterine fistula is an abnormal communication between the bladder and uterus, which communication, in the cases hitherto observed, has always been situated between the base of the bladder and some part of the canal of the cervix uteri. Such a condition, of course, implies a complete incontinence of urine, just as in the more common case of a vesico-vaginal communication, since the urine, after passing through the fistulous channel into the uterus, immediately flows away, through the os uteri, into the vagina.

Vesico-uterine fistulæ are of very rare occurrence. Very few cases have been recorded or noticed by British writers; and abroad I know of only one surgeon, M. Jobert de Lamballe, who has paid much attention to the subject. To this gentleman's researches, and to other recorded cases, I shall presently recur; but I prefer to commence by describing an example of this accident, which I have recently treated, and which is the only one in which, in this country, as far as I am aware, any remedial measures of an operative character have been resorted to.

C. R., a spare Irishwoman, aged 45, was admitted into St. Mary's Hospital, on May 9, 1862, under the care of Dr. Tyler Smith, who afterwards requested me to take charge of her. The following is the history she gave of herself:—She had been married four years. Four months after marriage she miscarried. Two years afterwards she was confined of a child at the full period. The labour was protracted, and she suffered severely, being finally delivered with instruments, but no bad results followed on this occasion. In January of the present year, four months previous to her admission into the Hospital, she was again delivered of a child at the full period. On this occasion she was in labour twenty-four hours, and again suffered greatly; but she was not aware whether or not instruments were again used. The child did not live. Immediately afterwards she found herself unable to retain her urine, and the whole of it had since escaped involuntarily, so that her clothing by day, and her bedding by night, had been constantly saturated with it. The labia and inside of the thighs were much excoriated by the continual irritation.

On examination with the speculum, there appeared to be nothing the matter with the vagina or urethra; the vesico-vaginal septum was perfectly sound throughout; but when the os uteri came into view, a small quantity of fluid was observed to flow from it into the cavity of the speculum. By digital examination, the os uteri was found to be somewhat larger than natural; it would just admit the tip of the finger, with which, about half an inch above the os, and in the canal of the cervix uteri, an opening could be felt on the anterior surface, leading towards the bladder. A sound introduced into the bladder by the urethra could, with a little manipulation, be brought into contact with the finger in the cervix uteri. The opening appeared to be rounded, and to be about the size of a pea, as if a small piece of the uterus had been punched out.

As regards the arrangement of the peritoneum between the bladder and the uterus, it may not be waste of time to say a few words. Supposing the average length of an unimpregnated uterus to be three inches, the upper two inches, or rather less, may be allotted to the fundus and body, and rather more than the lower inch to the cervix. The line of reflection of the peritoneum anteriorly, from the uterus to the bladder, takes place as nearly as possible opposite the junction of the cervix with the body, and is, therefore, at least an inch above the lower end of the organ. The neck of the uterus has been divided anatomically, into the vaginal and super-vaginal portions, the former being that small part which may be seen from the vagina; the latter, the larger portion situated above the insertion of the vagina, and between that point and the junction of the neck with the body. The whole of this latter part of the cervix is in immediate relation anteriorly, with the base of the bladder, without the intervention of peritoneum; and it is here that vesico-uterine fistulæ have, in every instance, been found.

The reflection of the peritoneum posteriorly is widely different, the whole posterior surface of the uterine neck, and also the upper three-quarters of an inch of the vagina being covered by that membrane.

From this brief anatomical digression I will return to the case in question, the nature of which I had accurately ascertained in the manner above described. It remained to determine if any, and what remedial measures should be attempted.

Two plans of operation, essentially different in their nature, had been suggested by Jobert, the only authority I know of who has paid special attention to the subject. The first plan which he suggests consists in making free lateral incisions through the os uteri and through the upper end of the vagina, so as to convert the lower part of the cervix uteri into two flaps, anterior and posterior, by separating which, the fistulous aperture might be brought into view, so as to admit of its edges being pared and sutures being applied. He suggests that these incisions should pass completely through the lower end of the uterus, and be continued into the upper end of the vagina, until they penetrate into the loose areolar tissue of the broad ligament. These incisions, he says, render it easy to get at the aperture of communication, and enable the surgeon to perform his manipulations with facility, and without any obstacle whatever.

This plan of procedure I must say I felt considerable hesitation in adopting. Free incisions into the uterine tissue are not, perhaps, attended with much risk—as modern practice has pretty abundantly demonstrated—but to penetrate into the loose cellular tissue of the broad ligament, seemed to me to be a rather serious matter. No one would, I think, be much surprised to find such incisions followed by peritoneal inflammation, or by diffuse suppuration in the pelvic and sub-peritoneal cellular tissue. Neither could I, from my experience in plastic operations high up in the vagina, accept without some reservation M. Jobert's statement as to the extreme facility of a similar proceeding in the interior of the uterus. He, however, records a case in which he operated in this way in the year 1849, and in which he ultimately succeeded in curing his patient, though a rather serious train of symptoms followed the operation, and the treatment was protracted over a period of several months. Three months after the operation an escape of urine took place through a small opening in the track of one of the sutures which had not been removed, but this was closed under the application of the nitrate of silver. Though the progress of this patient was, in many respects, tedious and unsatisfactory, and her life appears to have been in considerable danger, still, to have succeeded at all in so difficult a case, especially before metallic sutures had been introduced, was, undoubtedly, a great triumph in surgery.

I, however, thought it better, for the reasons above stated, to reject this mode of operation; and the idea which suggested itself to me to reach the fistula was to enlarge the opening, in the downward direction, by dividing that portion of the uterus which was situated below the fistula, and by continuing this incision into the upper end of the vagina in the longitudinal direction. I should thus have obtained a large longitudinal opening, the upper angle of which would have corresponded to, and have been formed by, the fistula itself. I believe that this would have enabled me to get at its upper end, where the fistula was placed, and which was the only difficult part to deal with, so as to apply the necessary sutures. If, however, I did not find sufficient room, I could have obtained more by making lateral incisions in the cervix uteri, in the direction indicated by Jobert, but of less extent. This plan would, doubtless, have been attended with considerable difficulty, still, I believe, it was not impracticable—the danger would not have been great; and, if I could have succeeded in closing the uterine portion of the wound in a first operation, there would have been comparatively little difficulty in dealing with the vaginal portion on a subsequent occasion. But there was the alternative of the second operation, suggested by Jobert in his treatise, but which I did not then know that he had ever put in practice. In consisted in closing the os uteri by suture, thus shutting off the communication between the uterus and the vagina, but leaving the fistula itself uninterfered with. When this was done, the urine would still, of course, get into the uterus, but it could not pass further; and the remedy, so far as the escape of urine externally was concerned, would be practically as perfect as if the fistula itself had been closed. Future pregnancy would, of course, be impossible, and menstruation must be performed through the bladder, by means of the fistulous opening; but these disabilities appeared, to my mind, to be more than compensated for (especially taking into consideration that the patient was 45 years of age) by the avoidance of all risk from the operation itself, by the greater certainty of success, and also by the fact, that should it not succeed, or should any difficulty in the matter of menstruation be subsequently met with, a slight incision through the new line of union would at once restore the parts to the same condition as before the operation, and either of the other procedures to which I have before alluded would have been as applicable to the case as ever; whereas, if I had commenced with either of the other plans, their failure would not have left the parts in nearly so favourable a condition for this more simple method.

Accordingly, I decided in favour of this operation, and on May 14 proceeded to put it in practice. The patient being placed in the lithotomy position, Bozeman's speculum was introduced, and held so as to depress the posterior wall of the vagina, and bring the anterior wall and the os uteri into view. The anterior wall of the vagina, just below the os uteri, was then seized with one vulsellum, and the posterior lip of the os uteri with another. By these means, with a little traction, the parts were drawn down within convenient reach, and were held in the required position by assistants. I then, with a narrow knife, freely denuded the edges of the os uteri, and brought the raw surfaces together with four silver-wire sutures, passed by means of the tubular needle. I brought the ends of the wires through the holes in a thin leaden plate, and fastened them with perforated shot, after Bozeman's method. I greatly prefer, in this and all similar cases, the use of the leaden plate to the fastening the wires by simply twisting their ends. The slight pressure which the plate exercises is of great service in keeping the part steady and motionless; and it also acts most advantageously in keeping the sutures throughout at a proper distance from each other, thus preventing that gaping apart of the wound in the intervals between the sutures which is so likely to take place if they become unduly approximated to each other. After the operation, a double curved silver catheter was retained in the bladder, and was connected by a piece of elastic tube to an india-rubber bag to receive the urine.

For three days after the operation, she suffered no pain whatever; her pulse was natural, and there was no sign of constitutional disturbance. No escape of urine took place, except through the catheter.

On the fourth day, she complained of pain in the back and lower part of the abdomen, her pulse was quickened, and there was slight thirst and feverishness. This was followed during the night by an escape of blood with the urine through the catheter, and the next day the feverish symptoms had disappeared. The discharge of blood with the urine continued for three days, and gradually ceased on the fourth. It was evidently the re-establishment of menstruation, which had not occurred since her confinement in January, four months previously. No discharge of blood took place per vaginum, nor was there any escape of urine, except through the catheter.

On May 21st, seven days after the operation, I removed the plate. The os uteri seemed firmly closed. There was no escape of urine. The catheter was retained for two days longer, when it was discontinued by degrees, at first for a short time only, afterwards for a longer period. At the end of a fortnight after the operation, she was able to dispense with it altogether, and had perfect control over her urine.

On June 4th, three weeks from the operation, she left the Hospital perfectly well. I saw her occasionally for three months after she left the Hospital; she remained quite well, menstruating regularly through the bladder at intervals of about three weeks. The passage of the menstrual secretion through the bladder was attended with no pain or difficulty whatever; on the contrary, she seems to consider it a very convenient arrangement.

On looking into the literature of this subject, the first recorded instance of a vesico-uterine fistula appears to be one given by Madame Lachapelle in her "*Practique de l' Art des Accouchemens*," published in 1825. This patient was delivered after a natural labour, but not till three days after the loss of the liquor amnii. Eight days after delivery incontinence of urine set in, and she was ultimately discovered to have a fistula four lines in diameter in the neck of the uterus. She was considered incurable. Another case which occurred in 1828 is given by Professor Stoltz, of Strasbourg. This patient died forty-one days after delivery, and a vesico-uterine fistula was found about the middle of the cervix; but there was also a communication posteriorly between the neck of the uterus and the peritoneal cavity, directly opposite the vesico-uterine communication. This patient appears to have died from peritonitis, caused by the escape of urine through this last-mentioned opening. A third case is that given by M. Jobert, to which I have already alluded, and which I believe is the first in which any operation was performed. It occurred in 1849, and will be found in his "*Traité des Fistules Vésico Uterines*," &c., published in 1852.

A very singular case is given by Dr. Simpson, of Edinburgh, in which an abscess formed between the bladder and the neck of the uterus, and opened a communication between the two cavities. Dr. Simpson dilated the os uteri, and applied nitrate of silver to the opening, under which treatment it gradually contracted and closed. (See Simpson's "Obstetric Memoirs," vol. i. p. 232.)

Dr. Leishman, Physician to the Lying-in Hospital, Glasgow, published an account of a case of vesico-uterine fistula in Octber, 1861, in the Glasgow Medical Journal. This patient had a lingering labour, and the liquor amnii was evacuated two days before the delivery was completed. Incontinence of urine did not commence till some days after delivery. In this case the incontinence gradually diminished. A year afterwards she was able to hold her water for four or five hours; and when the urethra was compressed, and she was directed to make an expulsive effort, no urine escaped through the uterus. The os uteri was healthy in appearance, and "firmly closed." It was remarkable that this patient, as the incontinence disappeared, had ceased to menstruate in the ordinary way, but had regularly, at monthly periods, passed blood with her urine. From this circumstance, together with the apparent closure of the os uteri, I should infer that the incontinence ceased, not because the fistula itself had closed, but because some spontaneous obliteration of the cervical canal had taken place below the abnormal communication; that nature had, in fact, performed the very operation which I practised myself in the case which forms the basis of this paper.

These were all the facts relating to this subject that I could at first meet with, and they were all that Dr. Leishman also, in his paper published in 1861, was able to collect. I, however, wrote to M. Jobert, asking him whether he himself had performed any other operations in similar cases, besides the one recorded in his book; and I was somewhat surprised to hear from him, in reply, that he had operated in thirteen cases, of which eleven were cured. They are all recorded, he informed me, in the *Lancette Médicale* and the *Union Médicale.* I have not been able to meet with any journal going by the first of these two titles, but I have searched the *Gazette* (or *Lancette*) *des Hôpitaux* and the *Union Médicale*, and in them I have found very circumstantial and elaborate accounts of seven cases, besides the one to which I have already alluded. (See *Gazette des Hôpitaux* for July 19th and August 20th, 1853; also *L' Union Médicale*, November 22nd, 1856, January 6th, 1859, three cases, and October 18th, 1860). After his first case, M. Jobert appears to have given up the attempt to get at the fistula itself by making extensive lateral incisions in the uterus and vagina; and to have resorted, as I did, to the easier and safer, though physiologically less perfect, operation of closure of the os uteri.

One of his patients died from peritonitis. In this case the os uteri had been much injured, and its lips were puckered and irregular. Free lateral incisions were made in this case to relieve cicatricial tension, and to these incisions most probably the fatal peritonitis owed its origin. All his other cases appear to have terminated satisfactorily, and the patients obtained perfect control over their urine, while menstruation took place regularly and without much difficulty through the bladder.

It is very remarkable, that in the majority of these, menstruation took place, precisely as it did in my case, within a few days of the performance of the operation, whereas, from the time of the formation of the fistula to the time of the operation nothing of the kind had been noticed. It appeared as if the continual stream of urine passing through the cervix uteri in some way or other interfered with the re-establishment of that function, but that the mere entrance of a small quantity of that fluid into the uterine cavity did not produce the same effect.

In one particular, the case observed by me differed from all the others, if the history given by the patient is to be relied on. She passed her urine involuntarily immediately after delivery; whereas, in all the others, the incontinence was not established till some days afterwards, and in several was preceded by the separation and discharge of a slough. The immediate escape would seem to indicate a laceration of the uterine tissue, rather than a sloughing from protracted pressure, which is the most common and probable cause of vesico-uterine, as well as of vesico-vaginal fistulæ. It might be, however, that in this case, the incontinence for the first few days was of a temporary nature, and depended upon debility and loss of power in the sphincter of the bladder following the labour, which power might not have been regained till after the separation of a slough, and the establishment of a permanent incontinence as the result of the fistula.

In conclusion, I have only a few words further to add. There may, possibly, be some who think it an almost unwarrantable proceeding, even though it may not be a dangerous one, to produce occlusion of the os uteri; and, by so doing, not only to divert the menstrual secretion, but to produce an absolute barrier to future child-bearing. No doubt, it is a thing which should not be undertaken lightly, or on insufficient grounds; but I hold that there was ample justification for it in such a case as that which I have related. The condition of the patient, while unrelieved, was such as to render her existence loathsome to herself and to all around her, and utterly to preclude her from gaining her living by any employment which would bring her in contact with strangers. Furthermore, even if child-bearing were held to be the principal end and aim of female existence, it is next to impossible that impregnation should take place with a continual stream of urine flowing through the os uteri, mixing with and intercepting the seminal secretion. It should also be remembered, that those in whom such a lesion has occurred have almost always passed through the ordeal of parturition, once at least, at imminent risk of their lives, and that they will gladly consent to any operation, which will not only relieve them from their present misery, but will have the further merit (as they will most probably consider it) of preserving them in the future from dangers such as they have already gone through.—*Med. Times and Gazette, Jan.* 10, 1863, *p.* 26.

90.—CASE OF VESICO-UTERO-VAGINAL FISTULA CURED AT ONE OPERATION.

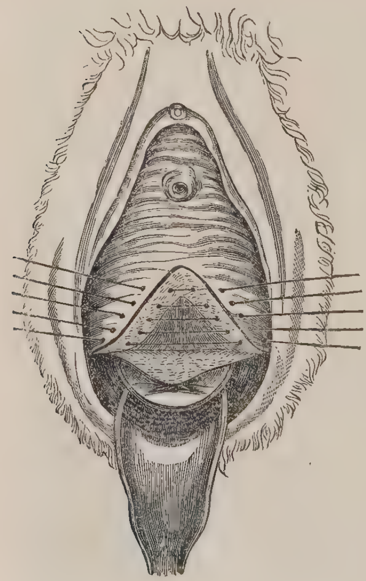
By T. S. Wells, Esq., Surgeon to the Samaritan Hospital.

The interesting paper on "Vesico-Uterine Fistula," by Mr. James Lane, published in the last number of the Medical Times and Gazette, induces me to bring the subject of "Vesico-Utero-Vaginal Fistula" also before the profession.

In Mr. Lane's case of vesico-uterine fistula a cure was effected by closing the os uteri, but the patient was thus rendered sterile; and, at each menstrual period, the bloody fluid from the uterus passed away through the bladder, mixed with the urine. As his patient was forty-five years of age, these conditions could not be considered as objections to the proceeding which he adopted; but they would render it altogether unjustifiable in a young woman, provided a cure could be effected without any such consequences. That it may be so effected, the following case is sufficient to prove. I am indebted for the notes to Mr. E. Parson, House-Surgeon to the Samaritan Hospital, a most intelligent gentleman who has just obtained the gold medal in Midwifery and the diseases of women, at the M.B. Examination for Honours of the University of London.

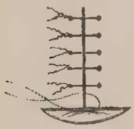
"J. W., aged thirty-nine, was admitted to the Samaritan Hospital on March 29th, 1862. She was married fifteen years ago, and has had nine children. The last was born three years ago; it was of an immense size. The labour lasted a whole day and night, and the child was born dead after the use of the forceps. The next day the urine began to pass through the vagina, and has continued to do so ever since, the usual misery connected with this condition rendering her quite unfit for her duties.

"On admission, her general health appeared to be good; but the labia, thighs and nates were much excoriated, and coated with phosphatic deposit. The urine was ammoniacal, and contained much mucus. At the distance of two inches and a-half from the orifice of the urethra there was an opening in the vesico-vaginal septum. A curved sound, passed through the urethra, was readily carried through this opening into the vagina, and a finger passed into the vagina went very readily beside the sound into the bladder; but the opening was hardly large enough to admit two fingers. It was triangular in shape, the apex towards the urethra. Its base was formed by the anterior lip of the cervix uteri, which was split into two portions by a fissure which connected the cavity of the bladder with the canal of the cervix for a distance of about three-quarters of an inch from the os. By careful washing of the external parts, the use of oil, and frequent syringing of the vagina and bladder, the state of the urine was much improved in a few days, and the parts became much less inflamed, so that on April 7, Mr. Wells proceeded to close the fistula by operation.



"Chloroform was not given. The patient was told to ask for it if the pain became so great that she wanted it, but she bore all without a single cry. She was placed in the semiprone position recommended by Dr. Sims, and the fistula was brought into view by his speculum. Mr. Wells first passed three silver sutures through the vaginal borders of the opening, using the wires to draw down the edges while he was paring them. This he did partly by a small knife and partly by scissors. While using the latter one of the sutures was accidentally cut; it was replaced, and a fourth and fifth passed. The annexed cut shows the appearance of the paired edges before they were brought together.

"The wires were all passed after the fashion of Dr. Sims, by a needle-holder and short needles carrying fine silk, by which the wire was afterwards drawn through. The edges of the fissure in the cervix uteri had been pared at the same time as the vaginal mucous membrane was removed, so that when the sutures were fastened—which was very readily done by simple twisting of the wires—a longitudinal line of union along the vagina came into contact with a slightly curved transverse section of the anterior lip of the uterus, as shown in the annexed cut. The apposition was so close that it seemed hardly necessary to apply a suture, but one was carried, as shown by the dotted lines in the cut, through each side of the vaginal line of union and through the cervix.



"Dr. Sims' catheter was introduced as soon as the operation was completed, and the patient was allowed to lie on either side, or on her back, as was most comfortable. The vagina was syringed twice a-day; the catheter was changed and cleaned night and morning. No urine passed by the vagina. On the ninth day Mr. Wells examined, and finding union perfect, he removed all the stitches, but kept the patient in bed for five days longer. When she got up she could retain the urine for two hours at a time, the whole passing by the urethra. After she had been moving about for a week, M. Nélaton carefully examined the woman with Mr. Wells, and found that union was perfect. In order to see if any small opening might still remain between the bladder and uterus, the bladder was injected with warm water till it was full; but though it overflowed by the urethra beside the catheter, not a drop escaped into the vagina. Still, after this experiment, she was for several days unable to retain the urine for more than half an hour at a time. On May 13th the note is: 'can retain the whole of her urine one hour. If she does not pass it then, there is a little dribbling from the urethra.'

"May 21. She was discharged, the power of holding the urine gradually increasing.

"A letter, dated September 29, speaks in most grateful terms of the comfort she now enjoys compared with her former miserable condition. She is able to walk about all day without any inconvenience; but when she is lying down in bed, if the bladder becomes distended, she is obliged to get up, otherwise she could not avoid wetting the bed. This slight inconvenience is probably due to the bladder not having quite recovered its original size, and may be expected to disappear in time."

It is not uncommon to meet with cases of vesico-vaginal fistula in which the cervix uteri forms the upper border of the opening; but these do not become examples of vesico-utero-vaginal fistula unless there is a communication between the bladder and the cavity of the uterus or the canal of the cervix. I have only met with one other case of true vesico-utero-vaginal fistula besides that reported above. This was in 1857. I succeeded in closing a considerable portion of the opening, taking great pains to avoid closure of the os uteri; but a small opening still remained, after several operations, in the cervix uteri, and the patient fell into the hands of a person who has acquired some notoriety by his treatment of vaginal fistulæ. He operated, but was so ignorant of what he had done, that he boasted of having cured a case in which I had failed; and even after it was proved to him, by the fact of the menstrual fluid passing through the bladder, that he had closed the os uteri, he published his egregious failure as a successful case. This patient was a young married woman, yet her os uteri was closed quite unnecessarily,—as the first case above alluded to sufficiently proves.

Two practical details in relation to the cure of vaginal fistula deserve a word of passing notice—the material of the sutures, and the use of the catheter. Since this case occurred I have been gradually ariving at the conclusion that fine strong silk answers even better than wire. It is passed much more easily and with finer needles, causes less irritation of neighbouring parts, and is removed much more easily, while union takes place quite as well as when wire is used.

With regard to the use of the catheter, a most troublesome part of the after-treatment, I believe it may be dispensed with altogether. The catheter certainly need not be left in the bladder, and I believe it need not be used to empty the bladder. At any rate, in one of my later cases, it was never once used after the closure of a fistula by five silk sutures, and perfect union resulted with less inconvenience to the patient than I had ever seen before. That the use of the catheter is not altogether free from danger, is proved by the frequency of catarrh of the bladder, persisting occasionally for months, after a fistula has been cured; and by one case of death which occurred at Brussels, where, although the fistula was cured, the patient died of peritonitis caused by the point of the catheter having caused ulceration, and perforated the fundus of the bladder.—*Medical Times and Gazette, Jan.* 17, 1863, *p.* 58.

91.—ON PUERPERAL FEVER.

By Dr. P. M. LATHAM.

I was called one Friday, at midnight, to visit a young married lady, aged twenty-one, whose state had given just alarm to her medical ateendants. Her pulse was too rapid to be counted, and so feeble that it could only just be felt. She made no complaint of pain; and no questioning, no examination by pressure of the abdomen especially, could elicit the least expression of it. The abdomen was soft, and very slightly distended. The tongue was quite clear and moist; the extremities quite warm. Moreover, there was perfect consciousness; and she expressed surprise at the apparent anxiety of those around her.

Here the only present symptom was the pulse countless from rapidity, and scarcely perceptible from feebleness. But there had been another symptom. On the previous Wednesday, at 3 p.m., suddenly and without warning, there had been a long and severe rigor. No reaction followed, when it passed away; the pulse became extrordinarily rapid and feeble, and it had so continued ever since.

At 10 a.m. the next day (Saturday), the state of things was still the same, except that the abdomen had become flattened in consequence of evacuations which had taken place in the course of the night. There was still no positive symptom, but the pulse most rapid and feeble. At 10 p.m. this symptom no longer remained. The pulse was imperceptible at the wrist. The hands were cold. Consciousness remained, and even intelligence; and there was evidently no feeling on the part of the patient that she was going to die. She died in the night; I did not learn at what time.

Observe: of this rapidly fatal disease (if disease it was) there were only two symptoms—the long and severe rigor, and the rapid, feeble pulse. But they were not, either one or the other, symptoms in the strict pathognomonic sense, being common to twenty diseases different in kind, and diagnostic of none. But symptoms they were, and both alike, of things incident to all diseases, and terrible when they occur. The inceptive rigor denoted the shock suddenly given to the powers of life; and the countless feeble flutterings of the heart, which followed it, denoted their surrender.

Now, as to the nature of the disease in this case, we must seek it in things prior to the inceptive rigor, since it certainly did not appear in what occurred afterwards. This lady had been confined eight days. The labour was favourable. But the placenta was retained, and had to be torn away. Still she had gone on favourably in every respect until the rigor. There was nothing more to be told of previous history; and this, and experience of the like, were enough to show the nature of the disease, and that it was puerperal fever. But shall I say "that it was," or rather "that it would have been?" The morbid element was taken up, and became a quick-working poison to the powers of life. The disease had either only just emerged and was cut short, or it never emerged all. The disease, so to speak, killed itself; it killed itself in the birth. Its first impression was a fatal shock; and there followed, not the symptoms proper to its own nature, but the one symptom of coming death—this countless fluttering of the heart.

I was summoned suddenly to a lady, about forty years of age, who was to all appearance brought at once within the grasp of death. She was pale and cold, and her lips were livid; and her pulse was of a countless frequency, and just perceptible, and no more. Her mind was clear; and she could just be heard to whisper a few articulate words, telling me that she saw all things in a mist, and did not well distinguish one object from another; and that she seemed as if she were going to be suffocated. This looked awfully like dying. And into this condition she had passed in the course of two hours.

I had no other expectation than that she would die immediately. But hour after hour passed away, and she still lived. Days and nights, even five days and five nights, between Tuesday and Sunday, passed away, and she still lived. She lived, but without the slightest appearance of rallying, and without the least token of any positive disease. The countless, scarcely perceptible pulse, comprised all the symptoms. Brandy, ammonia, ether, anything and all things that could stimulate, were given (for the power of swallowing remained), every ten, or every five, or every three or two minutes, night and day. But they produced no reaction. They kept the heart just moving, and that was all. The pulse was sometimes perceptible, and sometimes not; and several times, when it was actually gone, and all was over, as it seemed, life and a pulse were brought back again by a larger supply of brandy.

After five days and five nights thus spent, a change took place; and it was all at once. It was within a certain half-hour. As I sat by her bedside, I found her pulse had for the first time become distinct enough to allow me to count it. Instead of a confused flutter, it gave 120 distinguishable beats in a minute. It continued perceptibly constant to this number for one half-hour, without the help of brandy. Presently what was dusky became red, what was cold became warm; and so much of reaction brought so much of hope.

But how did this frightful mystery end? How did it clear itself up? The tongue became red, then intensly red, then papillated; and then a rash diffused itself over the neck and chest, then over the whole body. And all was consummated in a regular attack of scarlatina, which went through its course without any unusual or unfavourable incident, and the patient got well.

In the first of these two cases, there was the highest presumption *à priori* (perhaps the proof), that the puerperal poison had gained access to the blood, and had entered into deadly conflict with the powers of life. Had the conflict been less than deadly, it would have had its natural issue in some authentic form of puerperal fever, which would have had its own perils. In the second case, there was the positive proof, *à posteriori*, that the scarlatina poison, gaining access to the blood, did for five days and five nights keep up an all but deadly conflict with the powers of life. But life holding out, the disease appeared as the natural consummation at last.

Now these two cases have the force of crucial instances. They are experiments of Nature's own contriving, and exclude all fallacy in the result. They prove the true significance of the frequent pulse; viz., that it is not a thing of disease as belonging to its essence, but a thing of disease and of all that concerns disease only as being a sign and a measure of the degree in which life and the powers of life are then brought to feel and to suffer, and to languish and fail. And this is the interpretation which the frequent pulse will be found to bear in the whole range of diseases in which it occurs, and in all the conditions, few or many, simple or complex, with which it is combined.—*British Medical Journal, Jan.* 17, 1863, *p.* 53.

92.—THE ETIOLOGY OF PURULENT INFECTION.

M. Flourens, in a note presented to the Academy of Sciences, gives an account of some experiments he has recently performed. He took some drops of pus from the dura mater of a dog and applied them to the perfectly healthy pleura of another dog. The dog died at the end of thirty-six hours, and a double purulent pleurisy was found to exist, the pleura being filled with pus, while no other viscus contained it. Pus was also applied to the abdominal muscles of a perfectly healthy dog. It died in four days, an enormous infiltration of pus having taken place amongst the different muscles of the abdomen. Moreover, pus transported from the dura mater to the pleura of the same animal, caused death on the fifth day, the left pleural cavity being filled with pus.—*Med. Times and Gazette, April* 4, 1863, *p.* 359.

93.—EMBOLIA AND THROMBOSIS.

By W. Cooper, Esq., Bury St. Edmunds.

The subject of embolia, thrombosis, or fibrinous clotting in the blood-vessels during life, is one which of late years has much occupied the attention of the profession, presenting as it does a frequent source of extreme danger and sudden death. Many valuable contributions to the pathology of this subject have appeared from various scientific sources—from Virchow, Richardson, Walshe, Mackenzie, and lately from Dr. Humphry of Cambridge, in his valuable pamphlet On the Coagulation of the Blood in the Venous System during Life. But the theory of the causes of the coagulation yet remains a somewhat controverted question.

By way of adding the practical experience derived from a case or two which have come under my notice, I am induced to occupy your attention by a brief detail of them.

In October, 1860, I was called, in consultation with Mr. Cooper of Ixworth, to see a gentleman who had been under his care for about ten days, and of whose case I obtained the following history.

He was called to see the patient (who had been much out of health previously) in consequence of his having ruptured some of the fibres of the gastrocnemius, by kicking violently at a rabbit which got up close under his feet. He suffered great pain from the injury, which lasted for some hours. A bandage was placed on the leg, and he was ordered to keep his bed.

On the fourth day after the accident, he was suddenly seized with severe pain in the groin and leg of the affected limb (the right). A vast amount of swelling, hardness, and tenderness pervaded the whole of the leg, from the groin downwards. The pain continued severe for some hours. On the third day from this, a sudden attack of prostration came on, with tightness at the præcordia, slight dyspnœa, cough, and expectoration of what was supposed at this time to be pneumonic sputa. On the third day after this, as he continued to get worse, I was requested to see him, and found him in the following condition. He was complaining of excessive prostration, with small feeble pulse; skin soft, and acting well; complete absence of febrile symptoms; sensation of tightness at the præcordia; pain on the right side of the chest posteriorly; slight dyspnœa; frequent and troublesome cough, with expectoration of sputa differing from the rusty coloured sputa of pneumonia, being expectorated in separate dark red sputa, combined with a certain quantity of tenacious mucus, resembling the plum-coloured expectoration which occurs in pulmonary apoplexy and purpura hemorrhagica. There was some dulness on percussion at the base of the right lung posteriorly, with minute crepitation. The right leg was enormously swollen, and still painful. A very perceptible hardness existed along the course of the saphena and femoral veins; and, from a circumscribed swelling beneath Poupart's ligament, there was no difficulty in pronouncing the seat of obstruction to the circulation to be the junction of the femoral with the iliac vein. The temperature of the leg was diminished, and the skin presented a mottled and purple appearance; in fact, the condition of the leg was that which is usually termed phlegmasia dolens. Associating this with the peculiarity of the chest-symptoms, the condition of the sputa, the small pulse, the excessive prostration, and the præcordial tightness, I believed that the pulmonary artery of the right side was also plugged with a fibrinous clot; and with this view the patient was treated. The lung gradually regained its normal condition, and the leg became somewhat less swollen and painful; and he had so far convalesced at the expiration of about ten days as to be allowed to be taken out of bed. In the act of doing this, he was attacked with the most severe pain in the left groin and leg. The hardness, tenderness, and tension, and the obstruction to the circulation, were so sudden and severe that some fear was entertained lest more serious symptoms should supervene, and the leg should pass into a condition of gangrene. On the second day from this, the same symptoms were manifested in the left lung that had been present in the right; and for some time he was in considerable jeopardy. His convalescence was slow, and much protracted by the occurrence of a small varicose tumour on the lower lip, from which he lost a considerable quantity of blood. The hemorrhage was troublesome and difficult to control, and yielded only to repeated applications of chloride of zinc. Considerable œdema of the leg still continues, more than eighteen months having elapsed since the attack; and I am disposed to believe that the current through the deep veins has never been re-established.

About the same period, I attended a delicate female, aged thirty-five, in her confinement. Her labours were always complicated, and she generally gave birth to a dead fœtus. On this occasion she went on well until about the twelfth day, when, in attempting to get out of bed, she was seized with a sudden attack of what appeared to be syncope, fell on the bed, and was with difficulty recovered. On seeing her an hour or two afterwards, I found her, with a pulse scarcely perceptible, complaining of an excessive feeling of prostration, slight dyspnœa, and tightness in the chest. She was some days before she completely recovered.

A. M., aged thirty-five, was confined with her tenth child. Her labour was natural; and she went on well until the ninth day, when she was suddenly seized with faintness, sensation of sinking at the præcordia, singing in the ears, slight dyspnœa; and for some days afterwards there was cough, and occasional expectoration of bloody sputa. At the end of the third week, she had pain in the right groin and leg, and was confined to her bed three weeks with phlegmasia dolens.

In October 1861, A.M., a married woman, aged twenty-eight, was convalescing from an attack of diphtheria; and, on getting out of bed for the first time, she was suddenly attacked with extreme faintness, difficulty of breathing, tumultuous action of the heart, and great sensation of impending dissolution. Before I reached her, she had partially recovered, but was some time before she was again able to get about. Some time afterwards, when she had apparently entirely recovered, on moving across her room she fell suddenly down, and said she was dying. She was removed to bed, and shortly afterwards expired, retaining her encephalic functions to the last. An inquest was held, and a *post-mortem* examination made. The right pulmonary artery contained a large, firm, dark clot, plugging the whole calibre of the vessel to some distance. The left pulmonary artery was also plugged to some extent with a clot apparently broken down, more pale and firm in structure than that found on the right side, and adherent to the sides of the vessel, but softened in the centre. The right heart contained some fibrinous clots.

F. M., a lad twelve years of age, had a severe attack of diphtheria, followed by paralysis of the velum and muscles of deglutition, and of the arm and hand. He was extremely enfeebled, and recovered slowly. During convalescence, and in the act of getting up from bed, he fell back as if faint, and died. The *post-mortem* examination discovered the arteries on both sides to be filled with dark and firm clots. The lungs were congested; but there were no fibrinous clots in the heart.

H. G., a carpenter, aged forty, when at work was seized with severe pain in the thigh and leg. Considerable swelling and tenderness supervened, extending from the ankle upwards in the course of the popliteal and femoral vessels. A circumscribed hardness could be felt in the groin. These symptoms continued for some weeks, and were followed by a crop of bullæ just over the inner ankle. which coalesced, and resulted in a large varicose ulcer, that was long in being cured.

The history of all these cases was undoubtedly one of obstruction to the venous circulation, from the deposition of fibrinous clots; and some of them presented features of interest worth remarking upon. Amongst others, in Case 1 we would notice the similarity of the lung-symptoms to those of pneumonia, and the curious confirmation of the diagnosis by the recurrence of the disease in the opposite side of the body; and also, was the supervention of phlegmasia dolens in any way connected with the accident? In Case 2, the sudden attack of syncope, followed some time after by the occurrence of phlegmasia dolens, left no doubt of the nature of the first attack; and in Case 3 (the patient convalescing from diphtheria), the association of the first attack, from which she recovered, with the condition of the clot found in the left pulmonary artery after death, showing the partial restoration of the circulation, and her consequent temporary recovery.

The causes leading to this condition of coagulation in the cases I have related were clearly traceable, primarily, to the blood-condition. No previous history of rheumatism or heart-disease existed in any of them; and further investigation into the exact pathology of this disease will, I have no doubt, corroborate the view taken by Richardson, Walshe, Humphry, and others, that in nearly all these cases (not excepting phlegmasia dolens) the condition of the vessels themselves has no influence on the clotting process, except indirectly through their presenting facilities for the deposition of the clot at the junction of the different large veins, at the projecting angles of the confluent trunks, as well as by their valvular arrangement; and no evidence of any alteration in the internal structure of the vessels from any inflammatory process has ever been found; but that it essentially depends upon a cachæmia or altered blood-crasis, such as obtains in hyperinosis, following acute inflammation, in the cachexia subsequent upon exhausting disease of any kind, and more particularly in the puerperal and diphtheritic crasis.

The treatment I have found most available in these cases is that recommended by Dr. Richardson—the exhibition of carbonate of ammonia in large doses, upon the theory that the fibrine of the blood is held in solution by the presence of ammonia, and that its tendency to coagulation is increased by a diminution of the volatile alkali of the blood.—*British Med. Journal, Nov.* 8, 1862, *p.* 488.

94.—CASE OF EMBOLISM.

By M.M. Gouraud and Lancereaux.

An interesting case of embolia of the infundibulum of the right ventricle and pulmonary artery communicated to the Société Anatomique, of Paris, by M. Gouraud, has been made the subject of a report by M. Lancereaux. The following is M. Gouraud's *résumé*:—"A healthy woman, aged forty-six years, entered La Charité, for a fracture of the right leg, accompanied by considerable extravasation of blood. Scutter's apparatus was applied, and all went on well, the size of the limb lessening. After three weeks the apparatus was replaced by a starch bandage. On the following morning the patient was quite well, but, some hours later, violent palpitations of the heart occurred. the patient cried out, became livid, and was dead in a few minutes. On post-mortem examination, the right tibia presented two solutions of continuity, the fibula being fractured in only one place; there was an extravasation of blood infiltrating the whole thickness of the soft parts in this region. The veins of the right leg presented small coagula, which became more distinct and large in the femoral vein, the external and common iliac, and even in the lower part of the vena cava. The fibrinous coagulum was firm, elastic, of a deep red or rose colour, and was adherent at several points to the internal surface of the vessel. On the left side the limb and veins were healthy. From the lower part of the vena cava to the heart the blood was liquid. There existed in the infundibulum of the right ventricle and in the pulmonary artery a clot drawn out into the form of a leech, thirty-six centimetres in length, of a diameter much less than the vessel where it was found, of a rose or deep red colour, and not homogeneous. The lungs were engorged, but crepitant." M. Gouraud explains the obliteration of the passage by the arrest of the long clot, on arriving at a branch of the pulmonary artery, such as would not allow it to proceed further, and then by the ventricular contractions causing the other extremity to be folded back in the infundibulum, so as to lie opposite the sigmoid valves. It is necessary that we should abridge considerably M. Lancereaux's observations. The first question to which he applies himself is the cause of the coagulation which took place in the veins. He explains it thus:—The blood coagulated at the seat of the fracture necessarily compressed the mouths of the ruptured vessels; but, at the same time, coagula would form at the extremities of these vessels, and mount up, as is the rule for them to do, as high as the nearest valves. From the withdrawal of the *vis a tergo*, there would be stasis of the blood proceeding from the collateral veins, a new coagulum, commencing this time at the valves, and these latter coagula would lengthen gradually, and become, in their turn, the cause of the new coagula, until the principal venous trunk becomes completely obstructed. In this view the cause is a local one, namely, the diminution of the current of blood, and the influence exercised by the fibrinous clot upon the blood which surrounds it. Admitting the sufficiency of this cause to produce venous coagulation, other causes may be added, such as diminution or loss of contractile power in the veins, tumours compressing them, and whatever retards the venous circulation. General causes would also operate, on the one hand, by lowering the force of the heart and the contractility of the vessels, and, on the other, by causing modifications in the blood itself such as are even now little understood. It is important to point out that, under the influence even of general causes, it is always where the circulation tends to be slow that coagulation commences.

The clot, which begins to be formed at the situation of a valve, presents a form and characters which must first be treated of. At one extremity it presents the mould of one or two of the valves; its other end is rounded or conical, and upon its length may be perceived the smooth and clean impressions of valves. One of its surfaces, that in contact with the wall, is slightly striated, yellowish or marbled; the other surface, free and bathed in the blood, is brownish and granular. The length varies from some millimètres to several centimètres; its bulk may become considerable, since it generally forms in the largest vessels, and is thus the most frequent cause of sudden deaths. Besides, by reason of its characters, it constitutes the most positive evidence of embolia of the pulmonary artery when it is met with in this entirely valveless vessel. Observation teaches us that, where there is but one clot, and the death has been sudden, it is always the trunk of the pulmonary artery or the infundibulum which is found obstructed. The blood in the heart is ordinarily black and fluid, as in death by asphyxia. I do not, for my own part, think that a single embolus, arrested in one of the divisions of the artery, can bring about this fatal accident. For the most part the embolic clots are multiple, and always, I say again, if death has been rapid, they are found either in the trunk of the pulmonary artery or in its principal branches. As respects the smallest clots, they are rarely found in divisions of the fifth order, but mostly in those of the third or fourth. In some special cases known as capillary emboli, very small clots have been found in the smallest branches. The form of the migratory clots is generally cylindrical, their extremities at one time regular, smooth, and conical; at another, rough and torn; at another, only one end is torn, while the other is polished and conical. It is in cases where both extremities are smooth and untorn that valvular impressions are found upon the body of the clot, and one or two moulds of valves at one extremity. The clots which are torn at their extremities are generally devoid of impression and moulds, but they are now and then channelled. When one extremity only is torn, the other is generally conical. The same difference which we have established in the characters of venous clots is, consequently, found in the clots of the pulmonary artery; and there exists between the venous coagula and those of the pulmonary artery such a resemblance, that we are compelled to admit that the clots have been transported from the veins into this artery.

Besides these characters, embolic clots differ from coagula formed just prior to or immediately after death in their elasticity, brownish or marbled colour, and the condition of the fibrin which is always in progress of retrogression. The clots formed at death are soft, œdematous, flattened, branched, and only close incompletely the containing vessel. The clots which, during life, form primarily in the branches of the pulmonary artery (autochthones), differ from emboli in their form and seat, and in the absence of the characters which have been described. In certain circumstances, however, they are readily confounded with embolic clots, namely, where fibrinous coagula have become added to the latter, but it is always easy, by means of a section and examination with the microscope, to recognise the central embolus. If the bulk of embolic clots is very variable, their length especially presents great variety: thus, whilst some may only measure a few millimètres, others are several centimètres in length; such as I have seen produce sudden death, have been five centimètres long; that which M. Gouraud has described in his observation presented the extraordinary length of thirty-six centimètres. I am disposed to believe that some error has slipped into this measurement, especially seeing that the femoral and part of the iliac veins were filled with a fibrinous coagulum. Under these circumstances it is necessary to suppose that the embolic clot occupied primarily the greater extent of the vena cava, a hypothesis of little probability, since no symptom of such an obstruction was apparent during life. I am, consequently, driven to believe that some secondary coagulations have been comprised in the measurement. But be this as it may, it is certain that very long clots may be carried by the torrent of the circulation, and an important and peculiar character of them is, that they are curved and wound round, at one time in the trunk of the pulmonary artery, and in the infundibulum, as in M. Gouraud's case—at another, in one of the principal divisions of the pulmonary artery. But after a certain lapse of time these distinctive characters become wanting, and it is then very difficult to tell whether a coagulum, met with in the pulmonary artery, has been formed there, or has arrived there by migration. The only circumstance which we may thus be able to call up in favour of embolus, is the existence of a venous thrombus. The phenomenon which renders the embolus unrecognisable is important and really remarkable. The continued contact of the clot with the arterial wall determines a slight irritation, in virtue of which a blastema, exuded between the wall of the vessel, and the clot, soon becomes organised; by degrees, this substance extends on the circumference of the plug, and soon forms a sort of cupule, in which the latter is contained. At last it envelopes it completely, and encysts it, so that, after a time, often not very long, the fibrinous coagulum of the pulmonary artery is found to be everywhere surrounded by a perfectly organised membrane. Within this membrane, microscopic examination discovers an amorphous substance, more or less granular, embryo-plastic nuclei, elongated cells, and, above all, fibres of connective tissue. In the midst of these elements we sometimes find capillaries, free granules, the *débris* of red globules, and amorphous and crystalline hæmatin. After describing further changes in the condition of these encysted clots, M. Lancereaux proceeds to the subject of the condition of the lungs in cases of pulmonary embolia. It is evident that an embolus which closes up the trunk of the pulmonary artery and gives rise to sudden death, cannot cause any important disorder in the pulmonary parenchyma. Supposing such alteration possible, time would be wanting. But it is different when a coagulum comes to be situated in an important division of the artery, closing its canal completely. In this respect, M. Lancereaux expresses his agreement with Virchow, who states that, however complete the obstruction, it produces no alteration in the parenchyma, and, above all, no gangrene of the lungs. At the most, Lancereaux has observed slight diminution of volume, anæmia, or some œdema; and he explains this, physiologically, by the fact, that the pulmonary artery is an organ engaged in hæmatosis, and that the nutrition of the lung is effected, not by this, but by the bronchial arteries. Still (he proceeds to say) pulmonary coagula are sometimes accompanied by a lesion of the parenchyma of the lungs, whether they be the cause of it or not. Pulmonary apoplexy is frequently conjoined with obstruction of the branches of the artery; but it is to be remembered, that this generally occurs in the course of affections of the heart, especially in fatty degeneration; and it is also to be observed, that, under these circumstances, the clot is always situated behind the apoplectic spot, has none of the characters of an embolic clot, and is evidently autochthonic—not the cause, but an effect, of the apoplexy. The same thing may happen in certain cases of tubercular disease, of pneumonia, or even of gangrene. It is, however, important to notice, that there are certain special conditions of the embolic clot which are capable of giving rise to two of the alterations just alluded to—namely, pneumonia and gangrene. These conditions pertain to a special state of alteration of the tissues, in the midst of which the thrombus has been formed; if the coagulation has taken place in the midst of a purulent or gangrenous focus, the coagulum, formed in part of fibrin, and in part of other elements, possesses qualities in virtue of which it may alter the tissues with which it subsequently comes in contact; thus it is that metastatic abscesses often appear in the lungs of individuals, with suppurative thrombus of the cerebral sinuses, and in women suffering from metritis or suppurative phlebitis. Thus, too, gangrenous spots in the brain are found in persons who have primarily a gangrene of the lung; and gangrene of the lungs is met with frequently in paralytic individuals, in whom a sphacelus has formed over the region of the sacrum. Particles of fibrin or fragments of tissue, impregnated with pus or septic matters, become the points of origin of secondary foci, purulent or gangrenous, as the case may be.

Certain practical conclusions flowing from this fact related by M. Gouraud deserve attention. We find here a condition which has already been mentioned in other cases, one of which is related by Clinger. In three different cases sudden death has followed shortly upon compression exercised by a bandage upon the limb, which is the subject of the thrombus. It was, as we know, formerly customary to apply a compressing bandage upon a limb affected with œdema, or even with phlebitis, as soon as the acute stage of the inflammation had ceased. The practice is far from being free from danger, and it must necessarily be proscribed. But, besides, when, consecutively to a traumatic condition, fracture, amputation, &c., we have reason to suspect the existence of a venous thrombus, it will surely be prudent in the surgeon to abstain as much as possible from strong compression, if he would not expose his patient to more or less serious accidents. It is especially some time after the commencement of the coagulation, when the fibrin begins to disintegrate, that we must avoid this practice. And, for the same reason, every kind of handling of the injured limb should be avoided, and the most complete state of rest maintained. Indeed, in a certain number of cases, a slightly exaggerated effort, as I have seen on two different occasions, may suffice to bring about the separation of the clot, and sudden death. Occasional causes of this kind are marked, in the greater number of cases of sudden death, by embolia, and it is thus pointed out how necessary it is to be cautious when we have to do with patients suffering from venous thrombus.—*Medical Times and Gazette, Feb.* 14, 1863, *p.* 167.

95.—CASES OF PUERPERAL PYÆMIA.

By Dr. R. Barnes.

[In the following case communicated by Dr. Barnes to the Western Medical and Chirurgical Society, the patient was thirty-four years of age, and was confined of her eighth child on the 19th of August, 1860.]

All the symptoms went on well after the labour, the lochia being natural in quantity and quality. On the 23rd a very free eruption of *lichen simplex* occurred without any constitutional disturbance. In the course of four or five days slight desquamation of the cuticle formed. On September 1st and 2nd, the hands and body became covered with miliary eruption, which in the course of a day or two, dried up, except on the tips of the fingers and toes, and on the groins, where the vesicles formed large bullæ, several running together. These bullæ at first were filled with serum, becoming sero-purulent. All through she had generous diet, porter, bark, and ammonia. There was no fever, no pain, but great debility and depression. The lochia, however, for a day or two, became slightly offensive. On the morning of September 8th, she got up as usual, and seemed well, and was suddenly seized with a severe rigor, follow by general and complete collapse. I saw her during the rigor, and found her with a cold surface, anxious face, collapsed features, hurried respiration, and depressed circulation. Stimulants and warmth were freely administered, and, towards evening, there was slight reaction, the pulse becoming frequent, raging above 130, and very soft, and the body became covered with a profuse perspiration. Vomiting of bilious matter supervened, and intense pain, with but little tenderness, was complained of in the abdomen. The tenderness ran in the course of the descending colon, down into the pelvis, though the uterine region, properly so-called, was free. The tenderness was not at all commensurate with the pain. Bowels were regular, the tongue clean and moist, and the respiration completely thoracic and hurried. Ordered opium, brandy, beef-tea, fomentations, &c. On the 9th (following day) all the symptoms were increased, and she gradually sank on the morning of the 10th, about thirty-six hours after the first occurrence of the fatal symptoms. No *post-mortem* took place; the body rapidly decomposed. Upon carefully questioning, it appeared that there had been at an early period slight pains and swelling in the left leg; but it soon passed off, without any particular notice being attached to it. Of the nature of the disease there could be no doubt, and considerable interest was attached to the fact of the previous appearance of the miliary eruption with the copious formation of sero-purulent matter. The blood evidently was in a diseased condition, and nature had endeavoured to throw off the morbid element through the skin; but, failing this, the blood became more and more poisoned, until its pyæmic condition was manifested by the occurrence of the fatal symptoms. It must be remembered that the patient had complained, at an early period, of slight pain and swelling of the left leg, which probably was the first indication of mischief in the veins. From the position and direction of the pain, it is probable that the severe and fatal symptoms did not occur until the inflammation had extended to the left iliac vein; when it did so, death rapidly occurred. In another case of the same kind, the Author stated that, about the third day after delivery, the patient complained of pain in the calf of the right leg, which soon passed off, and the patient began to get about again, when, at the expiration of about twelve or fourteen days, without any warning, she became affected with all the severer symptoms of pyæmia. Stimulants, opium, &c., were freely (as in the other case) administered, and with success. Hence, in all puerperal cases, the necessity there is for attaching importance to all pains and swelling in the course of veins of the extremities, since, though slight at first, they may be the precursors of the more fatal symptoms, which we can scarcely hope to combat.—*Medical Times and Gazette, Dec.* 6, 1862, *p.* 616.

96.—OBSERVATIONS ON OVARIOTOMY.

By Dr. CHARLES CLAY, Manchester.

[The following is an abstract of a paper read before the Obstetrical Society of London.]

Dr. Clay gave a brief and interesting outline of his experience on this very important branch of surgery. Of 109 peritoneal sections, of which 104 were for ovarian extirpation, 3 for cutting down upon the tumour to establish ulceration where its removal was known to be impracticable, 1 for the Cæsarean operation, and 1 for the removal of both uterus and ovaries. Of the 104 ovarian cases, 72 recovered, 32 died; all the 3 ulcerative cases recovered; the Cæsarean section lived to the fifteenth day; and, lastly, the case of entire removal of both uterus and ovaries recovered. Of the 32 deaths, 10 died from the immediate consequences of the operation, 10 from inflammation, 10 from prostration, and 2 from hemorrhage. The great majority of the first and second series were young females, as well as a portion of the third division. Those from prostration were chiefly elderly females. Some other statistical facts were elicited, as well as the following remarks from the author:—Dr. Clay still defends the raised temperature of the room for operation, and attributes much of his success to its influence; is not certain if chloroform has added anything to the successful results, but values it highly as an agent which it would now be difficult to lay aside, although the first 14 of his cases were performed before it was discovered, and of which 9 recovered; and he still thinks, if a woman could face the difficulty without it, it would be in her favour The large incision is still practised by him, and deemed far preferable to the smaller opening. Of course the author wished to be understood that the incision was to be commensurate with the tumour to be extirpated. Dr. Clay gave many reasons for this preference. The distressing vomiting he conceives to be in a great measure owing to the use of chloroform, as he saw but little of it in the first 14 cases where it was not used. For this troublesome symptom he advises patience until the blood has got rid of its load of carbon, the simplest of drinks, and as little food as possible. Some very well ascertained facts of critical days were adduced, which would require too much space to dwell upon ; suffice it to say, the third, sixth, and ninth were the principal, and the causes of each were pointed out. No particular age seems to be prominent in respect to the success of these cases. Dr. Clay himself stated them to be about equally successful at all ages from sixteen to fifty-seven. Purgatives are not admissible ; and he relies on enemas with ox-gall, &c. This part of the paper was concluded by some interesting remarks on ovariotomy for the last twenty years, and the difficulties the author had to encounter, not the least of which was misrepresentation.

The author next gave in detail a new and interesting operation, which he believed to be the first of its kind, successful at least, in this country,—namely, the entire extirpation of the uterus and its ovaries through the abdominal walls, which has ended most fortunately, the lady returning to her friends on the thirty-fifth day after the operation, and still continuing well, thus establishing another great fact in reference to abdominal surgery. The case was that of a fibroid uterus of eleven pounds weight, with the ovaries in an unhealthy condition ; and the tumour by its growth had latterly so entirely filled up the cavity of the pelvis as to render the passage of the fæces and urine extremely difficult. The particulars of the case throughout its progress were given. Dr. Clay does not suppose that many uterine cases could be advisedly extirpated, but thinks some of those densely-hard fibroid masses, where the constitution has not been greatly prostrated, might afford a fair prospect of cure under the knife.

Mr. Baker Brown prefaced his remarks by warmly thanking Dr. Clay for the very practical and admirable paper just read. Mr. Brown considered it one of the most valuable he had ever heard, and wished it had been brought forward a few weeks earlier, as it was calculated to strengthen the hands of other ovariotomists, and enable them to contend against those who doubted the value of the operation. With regard to the temperature of the room, Mr. Brown stated that he believed that Mr. Lane, than whom no one had been more successful in ovariotomy, disregarded this subject, as did also another gentleman present at the meeting, who had also had great success.

For himself, Mr. Brown believed that where the operation was likely to be long, and the viscera long exposed, it was of great importance; but that where the operation was quick, and the viscera were kept back by hot flannels, the question of temperature would not affect the success. As to the long incisions, he believed Mr. Walne followed Dr. Clay; whereas Dr. F. Bird and Mr. Lane had contended for the short incision, and with good results. Mr. Brown always made a small incision first. If it appeared that on tapping the cyst the tumour could be withdrawn, he did not enlarge it; but if there appeared strong adhesions, or the tumour was very multilocular, he found it easy to make the incision larger, and agreed with Dr. Clay that it was advisable to see clearly where the adhesions and difficulties in removing the tumour were situated. At the same time, with the very large success of Dr. Clay, it was folly to say that short incisions were preferable to the long. Dr. Clay still preferred the ligature of Indian hemp. Mr. Brown had used it till the invention of the carpenter's callipers as a clamp, which he found more convenient and confortable. He could not, however, agree with Dr. Clay as to the inadvisability of bleeding. In two or three cases where peritonitis had come on quickly after the operation, he had found venesection most valuable, and attended with success. He always advised hot linseed-meal poultices to be applied over the whole abdomen when there was a chance of peritonitis, believing that they kept hot longer than ordinary fomentations. Mr. Brown's experience as to the frequency of the ovary attacked was contrary to Dr. Clay's. He had examined many hundreds, he might say, thousands of cases, but thought that one could not decide which ovary was diseased till an incision was made. His experience, from cases submitted to operation, was that the left ovary was most frequently affected. He agreed with Dr. Clay that age does not make much difference. He was glad to hear that Dr. Clay had come to the conclusion that the question of success did not depend on the operation itself, but on the after-treatment. Mr. Brown believed that in his earlier operations he lost more patients because the after-treatment was not so well understood as now. He had learned from Dr. Clay the valuable practice of never giving the patient anything but the most simple food till asked for. He preferred giving beef-tea and wine, if required, by the rectum for three or four days after the operation. Mr. Brown considered Dr. Clay's testimony valuable as to the great advantage of small over general hospitals for these operations, not on account of the operation itself, but because the nursing, ventilation, and and atmosphere were so much better in small special institutions. Mr. Brown had twice removed both ovaries with success. He thought that the question of exploratory incisions had been unfairly treated by the profession. They were made with an honest endeavour to ascertain the truth before risking the patient's life. Mr. Brown had never seen a fatal result follow them, and thought they should be commended and encouraged, not condemned. He was glad to hear Dr. Clay say that in his last operation he had made it a stipulation that it should be left to him to decide whether he should proceed after making an incision. As to opium, Mr. Brown had at one time been as great an advocate for it as Dr. Clay; but believed that it increased the sickness, and he now never gave it unless imperatively called for.

Mr. Spencer Wells said that he must not be supposed to undervalue the very useful paper of Dr. Clay, or to be ungrateful for the lessons which he had taught us all by his able advocacy of ovariotomy, if he (Mr. Wells) ventured to discuss two very important steps of the operation in which his own practice, and the practice in London generally, differed from that of Dr. Clay. Dr. Clay still advocated the long incision; and he still left the tied end of the peduncle and the ligature within the peritoneal cavity. He could boast of a success attending this practice of seventy per cent. of recoveries to operations, and as success was the best criterion in surgery, it might seem presumptuous to question the wisdom of any operative proceeding practised more successfully. But his (Mr. Wells') own experience had led him so decidedly to prefer the short to the long incision, and to keep the tied end of the pedicle outside rather than to leave it in, that he could not help suspecting that Dr. Clay's great experience in the operation had led him to success, in spite of a method which more recent experience had modified or corrected, and which men of less experience could not follow without great danger of failure. After long incisions, there was so much more exposure or escape of intestine during the operation, so many more serious symptoms after it, and so comparatively protracted a recovery, even in successful cases, that his (Mr. Wells') own experience had taught him to avoid any greater length of incision than was necessary for the exposure and removal of the cyst or tumour. Every inch in the length of incision appeared to add something to the chances against the patient, and in cases where he had the choice either of making a long incision and removing a tumour entire, or of breaking up a tumour and removing it through a small opening—even though ovarian fluid might unavoidably escape into the peritoneal cavity and require careful sponging for its removal—he would prefer this alternative rather than make a very long incision. So, in his experience, those patients in whom it had been necessary to leave the pedicle and ligature within the peritoneal cavity had suffered so much more after the operation, and their recovery had been so much more protracted than others where the peduncle had been kept outside, that he would always prefer to keep it out if he could, and so avoid the danger of absorption of the putrid matter of the strangulated stump, or the peritonitis connected with the effusion of fibrin thrown out to circumscribe the stump and ligature. It seemed probable that the frequent occurrence of peritonitis in Dr. Clay's practice was in some measure due to his manner of treating the pedicle; for in his (Mr. Wells') own practice, peritonitis was a rare accident. Of eighteen fatal cases, it had only had any important share in the fatal result of two; in all the others, shock or exhaustion after the operation, or blood-poisoning, having been the cause of death; while in successful cases he hardly remembered peritonitis in any patient where the pedicle had been kept out. As to the temperature of the room, in his earlier cases he had followed Dr. Clay's practice; but latterly he had found it better simply to have the room kept comfortably—not excessively—warm, and after the patient was in bed to keep a good fire burning, and a window open day and night. In the use of opium, also, he had learnt to avoid all excess. If there was pain or restlessness, it was given in moderate doses, and repeated if necessary; but some patients had recovered without taking a single dose, and others with not more than two or three doses. Sometimes it was given to secure a good night, even if there was no pain. With regard to the removal of uterine tumours by abdominal incision, it was only under the most exceptional circumstances—where the life of the patient was in great danger from hemorrhage or the effects of pressure—that such an operation as that so successfully performed by Dr. Clay could be justifiable. Pedunculated peritoneal out-growths from the uterus might be removed with moderate risk, and so might in-growths towards the uterine cavity, or vagina; but any attempt to enucleate interstitial fibrous tumours of the uterus, either by incision through the abdominal wall, or by incising the cervix per vaginam, was attended with such very great risk, that nothing but the most urgent necessity would justify the practice. He (Mr. Wells) said this rather as the result of his own observation, than as any conclusion suggested by Dr. Clay's successful case.

Dr. Clay, in reply, expressed his great gratification at the manner in which the paper had been received by the Society. With respect to the details of the operation, he adhered to the principles laid down in the paper. It was remarkable that many of the best recoveries after ovariotomy had taken place in his practice after making the long incision, and where the tumour was large he preferred it. The use of Indian hemp for ligatures he still preferred. With respect to the remarkable case of extirpation of the uterus, he would observe that on his way to town to attend the meeting he had accidently met the patient who had been the subject of the operation at a railway station, and in perfect health.—*Lancet, April* 11, 1863, *p.* 417.

97.—HISTORY AND PROGRESS OF OVARIOTOMY IN GREAT BRITAIN; WITH OBSERVATIONS FOUNDED ON PERSONAL EXPERIENCE OF THE OPERATION IN FIFTY CASES.

By T. SPENCER WELLS, Esq., Surgeon to the Samaritan Hospital.

(Read before the Royal Medical and Chirurgical Society.)

[Ovariotomy is an operation of British origin. It was first suggested by William Hunter, was warmly advocated by John Bell, and was first practised by M'Dowell, an American pupil of John Bell. The operation was first performed in Scotland in 1823, by Mr. Lizars, and was attempted by Dr. Granville in London in 1827, and successfully in 1836 by Mr. Jeaffreson of Framlingham. When the author began to operate in London in 1858, ovariotomy was at a very low ebb in professional opinion.]

The author then attempted to correct some errors which tended to retard the progress of the operation, showing that many of the alleged difficulties of diagnosis were easily overcome; and that, as after other capital operations, the better the general health of the patient, and the smaller the injury that is done in the removal of any diseased part, the greater is the probability of success. He proved that when patients recovered they were restored to perfect health, had lived many years, and had borne children of both sexes; and, in conclusion, left it to the Society to determine "whether an operation which has led to such results is still to be stigmatized as unjustifiable; whether they who perform it are necessarily open to the reproach that they do so rather to serve their own selfish purposes than for the good of their patients; whether they who, in the face of evidence sufficient to convince any unprejudiced mind, withhold from their patients a tried and approved mode of curing a disease otherwise incurable and certainly fatal, are not open to still more serious reproach; and whether it does not become us as men of science, who practise our art, not for our own advantage only, but with the earnest desire to do the very best that can be done for those who are confided to our care, and who trust in our knowledge, our skill, and our honour, no longer to condemn and oppose this operation, but rather to study its past history, to regard it with pride as an offspring of British genius, cultivated by British industry, and to assist its future progress by perfecting our means of diagnosis, and by investigating the the avoidable or removable causes of excessive mortality, reduce it to the comparatively low proportion to which it may be and will be reduced, and thus render ovariotomy in each coming year more honourable to British surgery, and more useful to mankind."

Dr. Graily Hewitt remarked that, in common with others interested in the treatment of ovarian disease, he had paid some attention to the subject with a view of arriving at a conclusion on the important question of the performance of ovariotomy, and he begged to submit some results of his inquiry into the matter. It always appeared to him that the first and weightiest question to decide in reference to the matter was the following one:—What is the natural history of cases of ovarian disease such as would be likely to be considered suitable for operation by ovariotomy where no such procedure is attempted? Upon the answer to this question turns the whole decision; and the question must be answered, seeing that the argument that such and such cases would not have ended fatally if they had been left alone is perpetually made use of by those opposed to the operation. It is evident that this question has been too little considered. Very few attempts have been made to arrive at some definite conclusion on this point. The difficulties involved in procuring the desired information are very considerable, and it can only be in the case of practitioners of long standing and experience, and who have been moreover in the habit of recording their experience, that it can be obtained. Even then the results procured are numerically limited, from the fact that many cases must pass under observation for a time only, and the record as to the mode of termination of the case is wanting. He had been able to procure but few data on the subject; but such as they were he would submit them. In a valuable series of Clinical Reports on Diseases of the Ovaries, published nine years ago, Dr. Lee, a much respected Fellow of the Society, had related his experience, and an examination of Dr. Lee's cases gave the following results:—Out of a much larger number of cases of presumed disease of the ovaries, there were 44 complete cases, respecting which the account given left no doubt that the cases were such as could be fairly compared with those which by individuals in favour of ovariotomy would be thought proper cases for the operation; that is to say, they were cases in which, from the size of the tumour, its physical qualities, such as fluctuation, the fact of tapping having been performed, or some other distinctive characteristic, it could be stated that they were cases of progressive ovarian disease. Cases were necessarily omitted in which the result of the case was not given, or in which the tumour was so small that the question as to ovariotomy would not have arisen. Now, the data afforded by these 44 cases as to the natural history of such disease gave the result that in 32 instances a positively fatal termination occurred, the cases being left to ordinary palliative treatment, tapped, &c. In 1 case the inference is that the patient died, in 1 the inference is doubtful, in 2 the patients were apparently dying, and in one other case the disease was "proceeding rapidly to a fatal termination." On the other hand, in 1 case the disease had not reappeared after a tapping 26 years previous, in 1 little progress had been made in 3 years, in 2 the patients were alive after a period respectively of 2 and 3 years, in 2 death took place from ovariotomy, in 1 death occurred from an exploratory puncture. Thus the 48 cases are divisable into two series. 84 per cent. of the cases died, no radical measures having been attempted, and 16 per cent. lived (including the cases of ovariotomy and exploratory puncture, which might perhaps more fairly be put into the other series) for a period of 2 or 3 years, so far as the report shows, and in one case for a period of 26 years. So far as he (Dr. Graily Hewitt) had found, there were no data afforded by other observers so extensive as those of Dr. Lee's. Of the 84 per cent. of cases with a fatal result, that result ensued, so far as can be judged from the report, within an average of a year and three-quarters. So far for the natural history of progressive ovarian disease. Next with reference to the results of ovariotomy. The experience of the last few years, which only could be fairly taken as representing what could be done with ovariotomy, showed a favourable result of 68 per cent., 66 per cent., 70 per cent., figures so high as this, in the results of those who had operated most largely. Comparing therefore the results of the palliative and of the radical treatment, so far as the imperfect data belonging to the former class of cases allow us to do, it appears that the figures stand in the proportion of 16 to 60 or even 70. He would repeat that this was a most important point to consider in deciding for or against the treatment of ovarian disease by ovariotomy, the question which must be determined in each case being, What are the chances for the patient of surviving the effects of the disease when palliative measures only are adopted as compared with the chances afforded after ovariotomy? He quite agreed with the author of the paper as to the great improvement that had been made in the diagnosis of the disease, and its bearing on the favourable or unfavourable issue of cases. One error in diagnosis which had been of late more frequently committed than any other was that of mistaking fibrous solid tumours of the uterus or ovary, for disease of the more ordinary kind. It would be generally possible to avoid this by considering the history of the case. Large fibrous tumours, whether of the uterus or ovary, are of slow growth, as compared with tumours more properly cases for ovariotomy.

Mr. Erichsen said he felt sure that all must have heard with deep interest the very valuable paper that had just been read. The results detailed in it not only reflected honour upon the author, but upon British surgery, in which ovariotomy undoubtedly originated. Dr. Graily Hewitt had related the lamentable consequences that result from leaving an ovarian tumour to take its own course, and he (Mr. Erichsen) knew nothing more distressing than to witness the misery usually entailed by the gradual and progressive development of such a disease. What resource did surgery offer for its cure except ovariotomy? Medicine had no effect upon a tumour that was an independent organism developing in the interior of the body. The minor surgical means, such as iodine injections, were more fatal than the extirpation of the growth. The opponents of ovariotomy were bound to answer this question—"If you reject that operation, what you have to substitute for it? But Mr. Wells had pointedly put it to this Society to state whether ovariotomy was a justifiable operation or not, and this question ought to be answered here. Now how were we to judge whether an operation is justifiable or not? By these tests—by the consequences of the disease being left to itself, by the mortality attending it, and by the result following its successful performance. They had just heard the lamentable consequences of leaving ovarian disease to run its own course, and it was known that medicine was inoperative in checking it. Now as to the mortality after ovariotomy. Several years ago, before ovariotomy had attained that degree of perfection and of comparative safety which improved methods of diagnosis and of operative manipulation have now given to it, Dr. Lyman, of New York, published a most able essay containing an analysis of 300 operations performed up to that time; of these the mortality was about 42 per cent. Since that period ovariotomy had greatly improved in safety, as was evidenced by the results of the operations of Mr. Wells, Dr. Tyler Smith, and others. The mortality now was probably not 30 per cent. Let this for a moment be compared with what occurred in other operations, which no surgeon hesitated for an instant to undertake. It was old and trodden ground to compare it with the result of the operations for hernia, ligature of arteries, &c.; and in these cases also the comparison was scarcely fair, as these were operations of necessity, whilst ovariotomy was an operation of expediency, and not of immediate and imperative necessity.

But compare it with "amputations of expediency" of the lower extremity. He (Mr. Erichsen) would take for this purpose the statistics of a most able paper published two years ago in the "Transactions" of this Society, giving the results of amputations performed in one of the largest hospitals in London—Guy's,—where it was known not only that the patients had the benefit of the most eminent professional skill, but all the appliances that wealth could afford for their recovery. In that very able paper Mr. Bryant stated that the mortality after amputation of the lower extremity for tumours was 36 per cent., and the mortality after "amputations of expediency" of the leg was 66 per cent. Compare this result of amputations performed under the most favourable circumstances, by men of the greatest skill and judgment, with those of ovariotomy, and the advantageous position of the latter operation would be at once seen. Then take the next point—the result of ovariotomy as compared with other operations for tumours, say of the breast. No surgeon hesitated to operate in cases of cancer of the breast, but he knew that the result was most unsatisfactory—certain and probably speedy recurrence; and he watched his patient with anxiety for the earliest signs of that return of the disease which he knew must inevitably occur. But how was it with ovariotomy? Why truly—

> "Aut cita mors venit aut victoria læta."

If the patient survived the operation her recovery was complete and permanent. There were doubtless a few exceptional cases, in which the disease might return in the other ovary, but these were so few as scarcely to deserve being taken into account. In the great majority of cases the patient was at once and permanently cured, without fear of recurrence. Considering, therefore, ovariotomy from all these points of view, it must be pronounced as a proper and a justifiable operation. There were some very important considerations connected with the diagnosis of ovarian tumours, and their conditions as affecting the probable result of operation, which deserved to be carefully studied. These were, the effect of concomitant dropsical effusions, of adhesions, and the influence exercised by the age of the patient on the operation. Errors had no doubt been made in the diagnosis of ovarian as in that of all other tumours, external or internal, but these were much less likely to occur, and did actually occur with much less frequency, at the present day, when the question of operation had perfected the diagnosis, than happened formerly, when so accurate a knowledge of the nature and conditions of these tumours was not necessary. With regard to concomitant constitutional mischief, as influencing the propriety of an operation, the surgeon would of course be guided by those considerations that influenced him in all cases. But there were two special complications which were of peculiar importance: one was ascites, and the other anasarca of the general cellular tissue. Ascites appeared to him (Mr. Erichsen) always to be a serious complication, and he did not much like to operate in such cases. Anasarca had been shown by Dr. Clay to be a condition unfavourable to operation, and in this he fully agreed. Then, as to adhesions. These undoubtedly were a serious obstacle to the success of an operation. They were an evil in two ways: first, by prolonging the operation, and sometimes causing great difficulties in separating the tumour from neighbouring organs. It was in this way that the intestine had been ruptured, portions of the liver torn away, the peritoneum stripped up from the abdominal wall, and other serious and fatal mischief induced. But adhesions were dangerous in other ways. They often contained bloodvessels which could not retract in the dense substance of the adhesion, and though they did not bleed much at the time of their division, did so afterwards, slowly oozing into the abdominal cavity, and giving rise to dangerous and fatal hemorrhage. But adhesions were not an unmixed evil. When extensive they seemed to obliterate the peritoneal cavity; to destroy the serous membrane as such, and to render it less liable to take on inflammatory action than when it was in its normal state. In this respect the peritoneum resembled the synovial membrane of joints. Just as we often see that when the knee has been chronically inflamed, its synovial membrane destroyed and replaced by fibroid tissues, a great amount of mechanical violence in the way of forcible extension and flexion may be inflicted upon it without exciting inflammation in it, so in the peritoneum, when that has been coated with old adhesions and organized plastic matter, inflammation is little liable to occur. Adhesions, therefore, were very dangerous and troublesome at the time of the operation itself, but rendered the patient less liable to the after-danger of peritonitis. Another important element in the success of the operation was the age of the patient. So far as his experience went, it was entirely in accordance with the deductions made by Dr. Lyman from his table of cases, that the operation was much more dangerous in young than in elderly women. Although not contraindicated by early age, it must be looked upon with more anxiety in young women from eighteen to twenty-five than in women beyond the middle age. The most successful age for its performance was after the cessation of the menstrual function, and every surgeon who had had experience in the operation must have been struck by the great success often occurring in apparently most unpromising cases, in women between fifty and sixty years of age. There were many other questions of the highest importance in connexion with this operation into which time would not allow him to enter; but on reviewing the whole subject, he (Mr. Erichsen) thought that the Society must reverse the decision to which it came ten or twelve years ago, and instead of condemning ovariotomy, give that operation its hearty approval and justification.

Mr. HUTCHINSON stated that he thought Mr. Wells deserved much credit for prominently bringing forward his facts respecting ovariotomy. He hoped that one great point would be gained by the recent progress in professional favour which this operation had made. In the future we might expect that patients would be operated on at much earlier periods than had hitherto been the case. To this, more than to any other change, he looked for an improvement in ovariotomy statistics. He agreed most fully with Mr. Erichsen in considering that the operation was a perfectly legitimate one. He had himself performed it in seven cases, with a result of four recoveries and three deaths. In an eighth case he had operated only on the afternoon of the day of meeting, and, therefore, could not include it. Now, as he had included several very bad cases in those operated on—one, especially, in which Mr. Erichsen assisted him, which was at one time almost beyond hope—he considered four recoveries a very fair result. He could fully confirm the statement of Mr. Wells, that when patients recovered they usually regained very excellent health, and were permanently restored to full enjoyment of life. As he did not believe that adhesions were of the slightest possible advantage, but, on the contrary, considered that their influence on the hopefulness of a case was entirely on the other side, he held it to be very desirable that cases should be operated on early, before they had time to form. In thanking Mr. Wells for the support which his facts gave to ovariotomists, he begged also to thank, not less warmly, Dr. Robert Lee, since he considered that the paper recently read by him, in which, for the first time, statistics were ignored by an opponent of the operation, was the greatest triumph which its advocates had yet achieved.

Mr. SPENCER WELLS said that the importance of the point raised by Dr. Graily Hewitt could hardly be exaggerated. No surgeon could be justified in performing any operation necessarily attended by serious risk to life, unless life was seriously threatened by the disease. If an ovarian tumour was not very large, or was not growing fast, no one would think of advising ovariotomy. It could only be justifiable when other treatment was useless, and life was threatened at no very distant period. But all experience showed that—putting aside some rare exceptional cases of women who lived for very many years with or without occasional tapping—it was rare for a patient to live for two years after an ovarian tumour had attained such a size as to raise the question of ovariotomy. He had stated his grounds for this belief in a paper read before the British Medical Association at Canterbury, and it was very interesting to find that Dr. Hewitt's analysis of Dr. Lee's own cases more than confirmed the accuracy of his estimate of the duration of life under palliative treatment. He did not agree with Dr. Hewitt in regarding fluctuation as being necessarily found in progressive ovarian tumours. He had seen many cases of such tumours, which grew very rapidly, and yet fluctuation could not be detected; especially if there was much fat in the abdominal wall. Nor did he think that it could be called a mistake if a surgeon removed a tumour, although he did not feel certain whether it would prove to be uterine or ovarian. The great questions were—"Is it movable, and can it be removed?" In two of his cases great doubt had been felt before the operation. One of them had been alluded to in the paper. The other patient had been examined by at least twenty gentlemen of great experience, and about half thought the tumour was uterine, and the other half ovarian. He himself felt great doubt after repeated examinations; and though, at the time of the operation, he was inclined to think it was ovarian, he would not have been surprised if it had proved to be a peritoneal fibrous outgrowth from the uterus. Had it proved to be so, it would have been removed quite as easily, and probably quite as safely as it was, though the patient made an excellent recovery. Provided the uterus and the tumour could be moved independently of each other, he thought the surgeon need not be deterred from operating by any doubt as to whether the tumour were uterine or ovarian; and he certainly could not be accused of a mistake if, before his operation, he felt the impossibility of being positively certain as to his diagnosis. Mr. Erichsen's support of the principle of the operation by comparing the mortality with that of other recognised operations was most valuable. With regard to anasarca, he had been led to regard it as an unfavourable sign, and had noticed that many of the patients in whom it had been present had done badly; but he thought we were arriving at a sort of law that, when it merely depended upon the pressure of the tumour retarding the return of blood from the lower limbs, it was of no more importance than in pregnancy. But when it depended on disease of liver, or spleen, or kidneys, or heart, or on leukæmia, then it should lead the surgeon to consider the case as unfit for operation. So with ascites. If it depended on disease of the liver, kidneys, or spleen, or on chronic disease of the peritoneum, the case was very unfit for operation. But if no such disease could be detected, and the dropsy seemed to be caused by the mere pressure of the tumour, or by its movements mechanically irritating the peritoneum, the effusion ceased as soon as the tumour was removed. He had seen several such cases do very well. In one there were fifty-seven pints of ascitic fluid, and in another forty pints. He could quite confirm Mr. Erichsen's statement that persons above fifty years of age recovered remarkably well. He had operated on seven patients between the ages of fifty and sixty, and only one died. But he did not think young women unfavourable subjects for the operation. He had twice operated successfully on girls of seventeen, and of fifteen cases between twenty and thirty, twelve had recovered. Between forty and fifty, of eleven cases, seven had recovered. But, between thirty and forty, of fifteen cases, only six had recovered. They seemed to do better before thirty or after forty, than between those ages. Whether this was accidental, and would be corrected by a larger number of cases, he could not say. He quite agreed with Mr. Hutchinson's remarks as to adhesions, although in many cases very extensive adhesions had not appeared to have any retarding influence upon recovery; and he concluded by thanking the Society for the attention which had been paid to the paper.—*Lancet*, *Dec.* 20, 1862, *p.* 685.

98.—SUCCESSFUL CASE OF OVARIOTOMY IN A WOMAN AGED SIXTY-FIVE.

Under the care of J. Hutchinson, Esq., at the Metropolitan Free Hospital.

[The patient was in otherwise good health, the symptoms were those ordinarily met with, but it was noticeable that the fluctuation was indistinct, and as if transmitted through very thick structures.]

On December 9th, ovariotomy was performed. An incision, just large enough to admit the hand, was made through the integuments down to the cyst. The hand being introduced, some slender adhesions over the whole front of the tumour were broken down very easily. The chief cyst was then punctured by a large trocar. When the cyst was nearly empty, it was drawn out of the wound, its withdrawal being very greatly facilitated by turning the woman over on her right side. It was necessary during the withdrawal to puncture two or three of the smaller cysts. The pedicle was long and thick. It was secured in a clamp, and the tumour was then cut away about three inches above it. The intestines had not been seen, Mr. Chance, who was assisting, having carefully kept the edges of the wound closed during the concluding steps of the operation.

The wound was closed by hare-lip pins, a flannel bandage applied, two grains of opium introduced as a suppository, and the patient returned to bed. The operation had occupied but a very short time, and the woman had borne it remarkably well.

On the 12th the clamp was removed, and on the 13th the pins were taken out. The wound had now entirely healed, but the stump of the pedicle was of considerable size, and occupied its lower end. Excepting a little trouble from vesical irritation at the end of ten days, and for which quinine and opium were prescribed, she had not the slightest ailment during her convalescence, and left the hospital in excellent health on the 8th of January, thirty days after the operation.

In commenting on the above case, Mr. Hutchinson adverted to the great assistance derived in ovariotomy from turning the patient over to one side. It was convenient to perform the first steps of the operation with the woman on her back, but as soon as the tumour was partially emptied the side posture had great advantages. The patient should be turned to the side opposite to that from which the tumour grew, as the intestines would thus be less likely to protrude. The advantages obtained are two—first in the greatly increased facility of dragging out the cyst, and secondly in the much diminished risk of any cyst-fluid finding its way into the peritoneal sac. With the patient on her back, and the cyst nearly empty, there is often still considerable difficulty in getting the tumour out, owing to the circumstance that the remaining fluid by its gravitation drags the cyst back again at one part as fast as the operator extracts it at another. All this is remedied by the side position, and the operation may be completed much more quickly, and with a smaller incision. The importance of preventing cyst-fluid or blood from gaining access to the peritoneal sac is obvious, and can scarcely be overrated. It is clear that there would be much less likelihood of any doing so with the patient on her side. Mr. Hutchinson added, that he had several times tried the side position, and found it more than realize the advantages expected. It had also been tried, at his suggestion, in a case in which he was not the operator, and with a result which was all that could be wished for. In cases of polycystic tumours, in which it was needful to cut into many cysts in succession, the side position was especially desirable, and in some such it might even save time to turn the patient well over, and then cut freely into the tumour, without any attempt at emptying it by means of the trocar. It was of course important to arrange for a good light from below in adopting this position.—*Lancet, March* 28, 1863, *p.* 358.

99.—ON OVARIOTOMY.

[About eleven years ago Dr. Lee published a paper entitled "An Analysis of 162 Cases of Ovariotomy which have occurred in Great Britain." The present article gives the opinions of the leading London men on the subject, on the occasion of a second paper read by Dr. Lee.]

Dr. Lee briefly states that his experience during the last eleven years, in regard to ovarian disease, convinces him that the published records of ovariotomy do not truthfully represent the statistics of the operation; successful cases having been made known, and the unsuccessful ones kept in the background. In none of the cases which have occurred under his own eye has he thought it right to recommend the operation; and he considers the slow progress made by many of the cases to have been a justification of the course pursued by him.

The President (Dr. Babington) said that fifteen years ago he saw Mr. Walne operate on a case of ovariotomy in a woman twenty-nine years of age. He made the large incision. The patient recovered, and was now well.

Dr. Tyler Smith expressed his surprise at the total want of argument exhibited in Dr. Lee's paper. As far as he could gather its meaning, it was a mere unreasoning declaration of hostility to ovariotomy, coupled with an approval of the do-nothing or palliative methods of treatment. Exceptional cases no doubt occurred, in which, either with or without repeated tappings, patients having ovarian disease lived on for many years in comparative comfort, and only slowly succumbed to the malady. But as the rule, it ran its course more rapidly, and towards the close the wretchedness and sufferings of such patients were extreme. In this unhappy condition Dr. Lee had nothing consolatory to offer, beyond the length of time over which their miseries might be extended. He (Dr. Tyler Smith) had, he regretted to say, for many years been of the same opinion, but had been gradually led to adopt the operation by the deplorable failure of all other means, and the favourable results of ovariotomy. A little more than three years ago he was consulted by a patient who had been under the care of Dr. Lee, and, thinking it a fair case, he suggested the propriety of the operation. He was informed that Dr. Lee condemned his diagnosis, and pronounced him a madman for daring to make such a suggestion. The patient, however, died, and a post-mortem examination showed that he had been correct. Since that date he had performed ovariotomy fourteen times. Of this number three patients had died, but eleven had recovered, and were now in good health. Two of them had been patients of Dr. Lee, and in both he had violently opposed the operation.

These two patients had recovered. He had not himself declined in any one case to operate because of the unfavourable state of the patient, and his cases, therefore, were fair cases. As regards diagnosis, he had failed in one instance in which he had attempted the operation. He had mistaken a case of cancerous tumour of the mesentery, of large size, complicated with ascites, for ovarian disease. After making an incision, the operation was abandoned. The patient was in urgent danger previously, and died within twenty-four hours. Such mistakes must, he supposed, sometimes occur; but they did not invalidate the general results. He believed the operation was more successful now than when Dr. Lee read his former paper, but even that showed the recoveries to be sixty per cent. He (Dr. Smith) was in favour of performing the operation as soon as the general health of the patient began to be threatened, but not earlier. Before this occurred, he considered the operation unwarranted, as it was impossible to know how long a patient might remain in tolerable health. There were, however, exceptions to this rule, as, for instance, when patients themselves insisted upon the performance of the operation. We had in practice to deal with mind as well as body, and to some patients the idea of an operation looming in the distance was intolerable, and could not be borne. Notwithstanding its formidable nature, ovariotomy was a more simple operation than its opponents believed. There was one circumstance which increased the mortality of the operation—namely, the opposition offered to it by authorities like Dr. Lee, which led patients to postpone the operation till they were in such broken health that it could no longer be performed with a fair chance of recovery. Without ovariotomy ovarian disease was confessedly incurable; but the cases of cure by the operation would soon be numbered by hundreds, and facts of this kind must, and would, prevail over the dictum of Dr. Lee and those who joined him in opposing its adoption.

Dr. Savage said that, as senior physician of the Samaritan Hospital, he had seen nearly all the cases operated upon in that institution by Mr. Spencer Wells, and, like Dr. Lee, had also got together a list of cases of ovariotomy; but his collection differed from Dr. Lee's in one important point. Dr. Lee never would see the operation done. He (Dr. Savage) had asked him to come and see a case, but Dr. Lee said he would rather not. This reminded him of an anatomist who denied the existence of the curling arteries of the uterus. Many years ago, when Dr. Savage was giving some attention to this point, he one day asked this gentleman to come and see these arteries in a uterine preparation; but he said, "No, he had said there were no such arteries, and he did not want to see them." Dr. Lee stood precisely in this same position. He had expressed strong opinions against ovariotomy, and he did "not want" to see anything which could alter his opinions. So his list of cases of ovariotomy, though large, was perhaps less reliable than his (Dr. Savage's), though only numbering between fifty and sixty, because he had taken care to become intimately acquainted with each case before the operation, during the operation, and after the operation. Thus his, being more precise personal information, might perhaps be accepted as making up for deficiency in numbers so far as to justify his offering a few remarks on the present question. When Mr. Spencer Wells became his colleague at the Samaritan Hospital, the authorities there, himself included, on the whole were unfavourable to ovariotomy; but Mr. Wells's success was decisive. Like Dr. Smith, he (Dr. Savage) could not help becoming a convert, but he could not agree in considering the extirpation of a diseased ovary a simple operation; quite the contrary. It seemed to him that there was no operation which could present a greater source of embarrassment, or required more presence of mind, readiness in resource, and the other best qualities of the surgeon. The reasons just advanced by Dr. Smith and Mr. Wells, in favour of ovariotomy, must be concurred in sooner or later, especially by those who had been in the melancholy situation of witnessing the progress and termination even of ordinary forms of ovarian disease. It had been noticed that the operation had been followed by an inequality of success, in regard to persons and places, which had excited a doubt, not only whether it could ever be brought under definite surgical rules, but whether individual statements of successful cases could be relied on. From his own observation, he could not help thinking that very much depended, not so much on the skill, as on the experience of the operator. For instance, what course would an inexperienced operator adopt in a case where, the tumour having been freed from the abdominal cavity in the most skilful manner, everything promising its speedy and successful separation, he could not find a pedicle—no place to apply any form of ligature? Would anyone, without some experience in such operations, be ready with a suitable expedient for such a complication? Yet he had lately been present when this occurred to Mr. Wells, and the measures resorted to were followed by one of the best recoveries. One of Dr. Lee's chief objections turned on the presumed insuperable difficulty of making out satisfactorily, in any given case, whether or not the tumour was a pregnant uterus, or whether pregnancy coexisted. This difficulty had not led to any mistake in any of the cases he had seen, nor could he imagine how it need be made. Fluctuation in many compound cysts was avowedly very obscure, and the abdomen, to the sight and touch, often closely resembled its appearance in pregnancy; but the usual modes of investigation are quite sufficient to make out a correct diagnosis in every case. He had come to the meeting in the anticipation of hearing sundry other points of difficulty connected with ovarian diagnosis discussed; but as what had transpired of Dr. Lee's communication offered no precise fact of any kind whatever, there was nothing to deal with. Dr. Lee had included in his list all the cases of ovariotomy he could get at: well then, the case he was about to allude to must be one of them, and therefore before the Society, and he thought it would be satisfactory to the profession if they were informed what were the precise points which led to the difficulty of diagnosis in a case which had occurred in the institution with which Dr. Lee was connected as physician accoucheur. The leading particulars of the case had been published, but he (Dr. Savage) thought Dr. Lee ought to explain, for their future guidance, how and why the difficulty had arisen.

Dr. Barclay said that, in speaking before the Society, something more than mere "hearsay" evidence ought to be adduced.

Mr. Spencer Wells regretted that the substance of the documents accompanying Dr. Lee's paper had not been made known to the meeting, because the portion of the paper which had been read contained neither facts nor arguments; and all that anyone could do, by way of reply, was to endeavour to prove that Dr. Lee's objections to ovariotomy should not lead to the condemnation of the operation. Three principal objections were stated by Dr. Lee. He said that women suffering from ovarian disease may live for a long time under palliative treatment; secondly, that it is often impossible to determine whether a tumour be really ovarian, and if so, whether it can be removed; and, lastly, that ovariotomy is a much more dangerous operation than published statistics would lead us to believe. In reply to the first of these objections, it would be admitted by everyone who had followed a case of ovarian disease to its natural or ordinary termination, that it was difficult to imagine a life of more hopeless misery; and that nothing could be more painful than to watch, day by day, a poor creature, who for some reason was beyond the aid of surgery, sinking into her grave, worn out by protracted suffering. Yet this was the fate to which Dr. Lee would condemn hundreds of poor women who might be restored to perfect health by ovariotomy. Then, as to the alleged difficulty of diagnosis, and the distressing mistakes which had been recorded, it must be acknowledged that these mistakes were errors of a bygone age. It would be almost impossible for any one acquainted with the ordinary practice of percussion to repeat the error of the earliest

Scotch ovariotomist, and open the abdomen of a woman whose only tumour was formed by flatulent distension of the intestines. Nor could anyone who had ever heard the placental murmur, or the sounds of the fœtal heart, repeat mistakes which had been made in the later stages of pregnancy. In the earlier periods of pregnancy, doubts would sometimes arise; and in cases where ovarian disease complicated pregnancy, an occasional error might be unavoidable; but, in the majority of cases, the diagnosis of ovarian disease might be brought as near to a positive certainty as could reasonably be hoped for in any department of surgery. From his (Mr. Wells's) own experience of forty-six cases in which he had performed ovariotomy, of six others in he had commenced the operation, or had made an exploratory incision, and of very many in which he had either simply tapped, or had injected iodine, he should say that the diagnosis was generally easy; and though in some rare cases it was not so, yet no important mistake had been made in any one of these cases. This alone was enough to prove that any supposed difficulty in diagnosis could not be maintained as an objection to ovariotomy; and the only logical conclusion which could be drawn from the mistakes due to the gross ignorance or gross carelessness of some surgeons, or from the very rare errors which might be unavoidable to the most careful and the best instructed, was not the condemnation of a useful operation, but the endeavour so to improve our means of diagnosis as to make errors less and less excusable. In no other department of surgery would the possibility of an occasional mistake lead to the abandonment of all surgical interference, and it would be quite as logical to decry lithotomy because a neither careless nor incompetent surgeon might, possibly, cut into a bladder which did not contain a stone; or to condemn the ligature of arteries, because some one had tied an artery to cure an aneurism which did not exist; or to raise an outcry against the excision of joints, because a joint had been cut out which afterwards appeared to be so little diseased that a few weeks' rest would have saved the limb—as to denounce ovariotomy because some surgeons who had performed it had made mistakes. Then, as to the mortality—undoubtedly it was high; but it was high because we are often driven to operate in cases where the patient had been so broken down by the disease that the hope of success is very faint. If only favourable cases were operated on, a very large proportion would recover. We were now gaining the knowledge which enabled us to say to a patient, "The chances are ten to one, or five to one, or two to one against you; or the chances of success and failure are about equal; or they are two to one, or five to one, or ten to one in your favour." Out of his (Mr. Wells's) own 46 cases, 17 had died and 29 had recovered, but many of them were very desperate cases; and he could say that he had scarcely ever lost a patient when he had felt very confident of success before the operation. Ten out of the last eleven cases had recovered. But it is said that the mortality is greater than the profession believe, because unsuccessful cases are concealed. This might be said of every other surgical operation. Men take pride and pleasure in their successes, and remember them, and make them known; while their reverses have often been so painful that they try to forget them. At any rate, they do not publish them, unless, for some special reason, they feel bound to do so. So in estimating the mortality of every operation—lithotomy, amputations, herniotomy, and so on—it is always necessary to make some allowance for probable error, due to the non-publication of unsuccessful cases; but with regard to ovariotomy, so much attention had been directed towards it, that we probably had a larger proportion of cases published, compared to the number of operations performed, than could be collected respecting any other operation. He (Mr. Wells) *knew* that none of his own cases had been kept back; he fully believed that many other operators had been equally truthful; and he would join most warmly with Dr. Lee in denouncing the conduct of any man who could bring forward his successful cases, and keep his fatal cases wholly or partly concealed. No punishment could be too severe for such flagrant dishonesty. But it was a libel on the profession to suppose that such an offence was common enough to throw discredit upon an operation which had done honour to English surgery. The Jury on Surgical Instruments in the International Exhibition have published their report—and a most able report it is; just what one would expect from the eminent men who composed the jury,—and among "the most remarkable additions to general surgery since 1851 which receive illustration in the present Exhibition," the jury thus speak of ovariotomy:—"Described by De Haen as an operation of which it would not do to talk, lest some reckless surgeon should attempt its performance; and by Scanzoni as a proof of madness in the patient who should adopt, and of crime in the surgeon who should abet, such a mode of suicide; and, again, energetically denounced by Velpeau as an operation on no account to be admitted into French surgery,—it is a source of legitimate satisfaction to English surgeons, from William Hunter downwards, that, thanks to their perception of of the conditions necessary to success, and their courageous self-reliance in the face of difficulty, an operation which, till lately, was considered scarcely admissible, should now be practised with results at least as favourable as attend many other capital operations." This "source of legitimate satisfaction to English surgeons" Dr. Lee would deny them; but it was to be hoped that the Society, so far from aiding him, would, on the contrary, influence professional opinion in favour of an operation which should rank among the greatest benefits conferred by surgery upon mankind.

Dr. LEE rose and said, it was true that he had never performed the operation of ovariotomy on the living body, that he had never sanctioned its performance, and that he had never seen it performed by others. In the year 1840 he had been invited by the late Mr. B. Phillips to be present at the St. Marylebone Infirmary to see him operate. He (Dr. Lee) consented to be present, but being professionally engaged, did not arrive at the Infirmary till the operation had been completed. The patient was twenty-one years of age, in good general health. An incision of two inches and a half was made through the abdominal parietes; the cyst was seized with the vulsellum; 330 ounces of fluid evacuated; the opening enlarged; the cyst drawn out; the root tied and excised, and the sac removed without difficulty. Severe pain followed, with vomiting. He saw the patient about half an hour afterwards with rapid, feeble pulse, and cold extremities. He was present at the examination of the body, when the appearances of recent inflammation were observed within the pelvis, with a small quantity of extravasated blood. He had seen the patient a few days before in excellent general health, and if her life had not thus been suddenly and violently destroyed, she might have lived for years. Mr. B. Phillips never again performed the operation of ovariotomy. He (Dr. Lee) was now told that ovariotomy was a simple operation, and the reports published of successful cases would lead to the inference that it was attended with comparatively little danger. No operation, he was convinced, could be performed on the human body so dangerous, except the Cæsarean section. Indeed the Cæsarean section and ovariotomy resembled one another in several striking respects. In both an incision must be made through the abdominal parietes, the peritoneal cavity laid open, and the bowels exposed. "I am acquainted," says a statistical writer in the thirty-fourth volume of the "Medico-Chirurgical Transactions," "with 409 authentic cases of the Cæsarean section, 341 of which are collected in Kayser's valuable essay, 'De Eventu Sectionis Cæsareæ.' In 251 of these cases the mother died; in 156 she survived. There can, however, be no doubt but that these figures convey a very exaggerated impression as to the proportion of recoveries, and that the unfavourable estimate of English authors is nearer the truth. Both Kayser and Naëgelé regard the results given by the published cases as *unfair*, and the former mentions the fact, which of itself affords strong evidence on this point, that while the total maternal mortality amongst the cases which he had collected was 63 per cent., the mortality of cases occurring in lying-in hospitals, in which institutions failures must of necessity be reported as well as successes, amounting to 79 per cent." The results here stated were admitted to be *unfair*, he would say wholly destitute of truth, and utterly unworthy of credit. Did any one person there present believe that of these 409 cases the mortality was not greater than 63 per cent.? It was known that numerous fatal cases of Cæsarean section had occurred on the Continent of which no report had ever been permitted to see the light. This applied forcibly to the statistics of ovariotomy, which he had been told were the sole foundation upon which all their conclusions respecting the propriety of removing ovarian cysts and tumours must rest. If all the fatal cases of ovariotomy had been published there might have been some appearance of force in this; at least the degree of danger would have been indicated. But this had not been the course followed in this country since the operation came to be performed. It was notorious that numerous fatal cases of ovariotomy had occurred, of which no report had ever been published, and all attempts to remove the veil which concealed them had been fruitless. "You have related, Sir, to the Society (addressing the President) a successful case of ovariotomy performed many years ago by Mr. Walne. Can you inform the Society of the number of cases in which he performed the operation with disastrous results, of which no account was ever published?" When preparing his (Dr. Lee's) analysis of 162 cases which had occurred in Great Britain for this Society, he wrote to Mr. Walne, and requested him, on the grounds of science and humanity, to communicate a full report of all his successful and all his fatal cases. With this request Mr. Walne refused to comply in the most peremptory manner, and no accurate report had ever been published of the results of his practice. It was long since Mr. Walne had been heard of as an ovariotomist; but the reports of success in his early career were quite as flattering as those marvellous results which had been related to the Society that evening. Mr. Walne was not the only ovariotomist to whom he (Dr. Lee) applied for information on the occasion without success. Another, whose fame as a successful ovariotomist had not been surpassed in this country, made a return to him, which was not correct, and which he was compelled to reject as untruthful. Of this ovariotomist little had been heard of late years, and he (Dr. Lee) believed he had abandoned the operation altogether. The postscript to Dr. Lee's paper contained an account of all the cases operated upon by Mr. Lane, Dr. Clay, Mr. Spencer Wells, and others, and the history of some fatal unpublished cases. The Council of the Society had decided that these should not be read. He had requested the last case in his postscript to be read, but that request had not been complied with. It was a case related in a letter to Dr. Noble. The operation was performed in 1855, with the sanction of Mr. Harrison's colleagues, and in the presence of Dr. Noble and several other medical men and Dr. Clay. "I removed the tumour," says Mr. Harrison, "and the first time I suspected it to be uterine was on cutting through the pedicle." The patient had died in eight hours. He (Dr. Lee) was uncertain if this case was published. Another letter in the postscript contained an account of three fatal cases. One of these had been published as a fortunate case. The histories of many others had been communicated to him, not referred to in the postscript. He had himself seen a considerable number of cases where the operation was performed, contrary to his advice, with rapidly fatal results, of which no reports had been published by which the cases could be recognised. He passed the house of one of those patients that afternoon. There was hope in this case that, with ordinary treatment, the patient might have lived for years. The operation was very lately performed with rapidly fatal results upon a lady he had seen in the country on the sea-coast. By no efforts had he succeeded in bringing to light the details of that case. If the operation was so simple and so successful, why were these cases concealed from the public and the profession? He (Dr. Lee) saw a case near Brixton some time ago, since the publication of his "Analysis." There was a great mass of ovarian cysts and tumours firmly adhering. He never saw a more unfavourable case for an operation; but a most marvellously successful ovariotomist was called to see the patient, and he pronounced it a favourable case. He said that he (Dr. Lee) was wholly ignorant of the subject. This ovariotomist made an incision from stem to stern, not an exploratory incision, but nothing could be removed after repeated attempts, and the patient was soon in her grave. Dr. Lee wrote to the ordinary medical attendant some weeks after, inquiring what had become of her. He received no reply of any kind. He wrote a second time without success. Many months after, Dr. Lee accidentally met a clergyman at Clapham, who informed him that the operation had been performed with speedily fatal results, and that he had buried her. It would be useless to multiply such cases; and yet he was told that the statistics of ovariotomy were to form the groundwork of all decisions respecting ovariotomy, and that they were worthy of trust. An attempt had been made to institute a comparison between the results of ovariotomy and great surgical operations. A chronic ovarian disease, after lasting for years, and not threatening life, bore no resemblance to a case of strangulated hernia, an aneurism ready to burst, or a shattered limb with hemorrhage from the large arteries. Ovariotomy could not be compared to any of the great operations of surgery. In the cases of ovariotomy called unsuccessful, the patients had been suddenly deprived of life by violence without any necessity. But it was not from some occasional cases of recovery from the operation, such as those related that evening, that a correct opinion of ovariotomy could be formed. All the facts must be taken into account, otherwise no sound judgment could be formed of the difficulty of the diagnosis and the danger of the operation. It appeared from his analysis of 162 cases, that a successful instance occurred in America, and was published in the Edinburgh Medical and Surgical Journal in 1822. He had thought that this was the first case; but it was now stated that the operation had been performed by Dr. M'Dougal before. This was a matter of no importance. It could not be denied that the publication of a successful case took place in 1822; and he had supposed Mr. Lizars, in consequence of this case, had recourse to operation. In Mr. Lizars' first case there was no ovarian disease to be removed. In the second, the disease was removed, but the other ovary was diseased and could not be removed. This patient was afterwards seen by some surgeons in London. He believed Mr. Lawrence saw her, with a great cicatrix of the abdomen, and a large mass of disease within. The third patient operated upon by Mr. Lizars died. In the fourth case there was no ovarian disease to be removed. About 1829, a case similar to this was seen in Guy's Hospital by Dr. Gooch. In 1826, Dr. Granville made an incision of nine inches long through the abdominal parietes, and a large ovarian tumour was brought into view, which had such extensive adhesions that it could not be removed. In 1829, Dr. Granville operated again, and removed a tumour weighing 9 lbs., which was supposed to be ovarian, but it was a large fibrous tumour of the uterus. Dr. Lee examined this tumour in the recent state, and ascertained that it had adhered to the fundus uteri by a thick peduncle, around which a ligature had been applied and the root cut across. A portion of small intestine which had come in contact with the incised root became inflamed and gangrenous, and the patient soon perished miserably. This preparation of the parts was long in the possession of Mr. North. Dr. Granville was about to operate upon another patient, but Mr. C. Clarke recommended the patient not be submit to the operation, and she lived some years after, and died a natural death at Barnes. Dr. Scott examined the body, and presented the tumour to Dr. Lee, which was the uterus with a fibrous tumour embedded in its walls of the size of three human heads; it was in Dr. Lee's collection at St. George's

Hospital. Dr. Granville had far greater experience in the practice of midwifery than any who had that evening spoken. In 1835 Mr. Jeaffreson performed the operation, and the patient recovered. From that time to the present the operation had occasionally been performed with success, but as often, he (Dr. Lee) believed, with fatal results, if the whole truth were revealed. About the same time Mr. King attempted to perform the operation, but the ovarian disease could not be removed, and the patient died. In 1834 he repeated the operation with success, and again in 1836. Then followed three successful cases and one fatal. Then followed six fatal cases running, in which the operation was performed by Messrs. Hargraves, B. Phillips, Aston Key, B. Cooper, and Mr. Greenhow. In looking over one column of his "Analysis," he (Dr. Lee) could see "Died—died—died—died—died—died" in a few days—he might say killed. All these operations were performed by distinguished surgeons, and none of them repeated the operation. In fact, the operation required undoubtedly great hardihood. So far it was an operation such as had been described. He would refer those who wished to know all the facts to the remainder of the "Analysis," by which it appeared that of 162 authentic cases in which the operation had been undertaken, in 60 the ovarian disease could not be removed, 19 of which proved fatal; of the remaining 102 cases in which the operation was completed, 42 terminated fatally. From these facts he concluded that ovariotomy and the Cæsarean section were the two most dangerous operations which could be performed on the human body. The postscript to his (Dr. Lee's) paper, which the Council had refused to read, contained reports of all the cases, successful and fatal, which had come under the care of Mr. Spencer Wells, and he believed that that gentleman had concealed no fatal case. One of the successful cases he (Dr. Lee) had seen in Burton ward, St. George's Hospital, and he thought the case as favourable for the operation as any he had ever seen. He summoned a consultation of the surgeons; but they declined to operate unless he sanctioned the operation, which he could not do, knowing that until the abdomen was laid open it was impossible to tell whether the cyst could be removed or not, and knowing also that the patient's life would, under the most favourable circumstances, be exposed to the utmost danger. She went to the Samaritan Hospital, and was operated upon by Mr. Spencer Wells with success; but she had a narrow escape with her life, and she told him (Dr. Lee) that she considered herself on the brink of the grave during several days. But the perusal of the successful cases of Mr. Spencer Wells had no doubt led to most fatal results. On reading some of these cases, a lady in Ireland, who had ovarian disease, resolved to have it extirpated, being convinced that ovariotomy was not attended with much danger. A pecuniary negociation took place between her and Mr. Wells, but it came to nothing; and another ovariotomist went to Ireland, and performed the operation. He (Dr. Lee) had been informed that he represented the case as not unfavourable, and that his fee was to be 300 guineas, and 100 guineas every day he remained with the patient after the operation. Bargains of this description he (Dr. Lee) had been informed were not uncommon. The operation was easily performed, and the operator ran round the table, kicking up his heels in triumph; but these feelings of delight were of short duration, for the patient soon began to sink, and died in eighteen hours. Had Mr. S. Wells ever read any report of this case? It was impossible to deny that the question now under discussion was a money question, and not one of science and humanity. Mr. S. Wells had reduced all his cases under three heads. One of these comprehended all the cases in which what had been called exploratory incisions had been made, and these were spoken of as if they were things of no very serious importance, though they had sometimes caused death. "You would not, I am convinced," concluded Dr. Lee, "view them in this light if incisions were made through your abdominal parietes, and the fingers of an ovariotomist introduced amongst your bowels to hunt for adhesions. The late Mr. Liston had a great horror of such exploratory incisions and of all ovariotomists. He was accustomed to call them 'belly rippers,' with a B before and a B after. The meaning of these two B's I must not state plainly to the Society."

[Having placed this important discussion before our readers, we append the comments of one of the Editors of the Lancet, as follows.]

Practitioners have long been divided with respect to the value of ovariotomy as a surgical proceeding. Is any further light thrown upon this subject by the discussion in question? We can scarcely think that there is. The tendency, on the one side, was to over-estimate its value; on the other, unjustly to depreciate it. What is really wanted is a more clear description of those cases in which the operation may be safely practised. That we are deficient at the present time with regard to the diagnosis of these cases the impartial observer will admit. The way to improve our knowledge on this point is not by wholesale assertions as to the facility with which we can determine which are the proper cases for operation, nor, on the other hand, can a general condemnation of the proceeding, and an exaggeration of the difficulties of forming a correct opinion, have any other effect than to involve the question in still further obscurity. It cannot be admitted at the present moment that our acquaintance with the nature of ovarian disease is so complete as the advocates for ovariotomy assert, or that we are so ignorant respecting it as the opponents of that operation so unhesitatingly contend. We think it is to be regretted, as the Council of the Society determined that the "facts" which Dr. Lee had forwarded should not be read, that the other portion of his paper should have formed the text of the discussion. Without these facts his communication amounted to mere assertions and objections to the operation, and gave his opponents a vantage ground which was scarcely fair to the veteran obstetrician. The Council, no doubt, had good reasons for withholding the letters which Dr. Lee had made the appendix to his paper, and which, like the postscript to a lady's epistle, formed in reality the more important part of his communication. One result, however, of the recent discussion will not be without benefit. It will show that whatever has hitherto been the success of ovariotomy in the hands of certain operators, the diagnosis of ovarian disease is by no means without its difficulties, nor the "simple" operation without its complications and dangers. The truth, as is usually the case in such questions, lies probably between the two extremes. Under favourable circumstances, and in well-defined cases, ovariotomy is a valuable operation. That it may sustain its reputation, it is essential that its advocates should be less sanguine in expressing their opinions. It is a duty not less incumbent upon the opponents of the operation to examine with candour the evidence in its favour.—*Lancet, Nov.* 22, 1862, *pp.* 566, 572.

100.—OVARIAN DROPSY.

A very interesting case has just occurred, where Drs. Warren and Storer, of Boston, U. S., were consulted, in which an attempt had been made to relieve the patient by the usual method of tapping; but, on introducing a trocar and canula, no fluid was obtained; the operation was repeated in another place, but with the same result. The tumour then increased to an enormous extent, the lower extremities were much swollen, there was difficulty of breathing and increased difficulty of locomotion. A very large trocar was thrust in below and to the left of the former punctures; no fluid followed the withdrawal of the trocar, but a feather, which was passed in through the canula about five inches, was found, on withdrawal, to be covered with a coloured gelatinous fluid, which was too thick and stringy to pass through the canula. After this her symptoms continued to grow worse, and the patient died in about five days, rather suddenly. On opening the abdomen, a thin sac was found filled with innumerable cysts of an oval shape, and resembling the shelless eggs found in poultry, varying in size from the egg of a sparrow to that of a goose. They were full of a substance resembling calves'-foot jelly, which in the smallest was clear and transparent, in the others more opaque, and in some yellowish. Most of the cysts were covered with a delicate net-work of minute vessels. The contents of the tumour filled two large water-pails, and weighed thirty-six pounds. The failure to obtain fluid upon puncture was very easily accounted for, not only from its gelatinous nature, but by its being contained in cysts.—*Lancet, Nov.* 29, 1862, *p.* 606.

101.—ON SOME RESULTS OF IMPERFECT DELIVERANCE IN ABORTION OR LABOUR, ESPECIALLY FETID UTERINE DISCHARGES.

By Dr. J. MATTHEWS DUNCAN, Clinical Lecturer on the Diseases of Women, Edinburgh.

Every obstetrician is well aware that the discharges from the genital passages of women are excessively fetid in a number of different circumstances. All discharges from these passages have a disagreeable, strong animal odour. To this I do not now refer, but to a putrid smell or fetor. There are varieties in the character and diffusibility of such fetid smells; that, for example, of cancer of the cervix differing from most of those resulting from mere putridity. But such differences, however interesting to the practical physician, cannot be well described in words, and can only be learned by frequent experience.

Among the causes of fetor in the vaginal discharges of women may be mentioned the retention in utero or in vagina of menstrual fluid, an occurrence not unfrequent in some women; the retention of lochia; the retention of leucorrhœal or purulent discharges; the retention in utero or in vagina of the ovum at different degrees of development, or of parts of it; the retention of decidual membrane; cancer of the vagina or uterus; extra-uterine pregnancy; disintegrating fibrous tumour; abscess of the genital organs, or in the pelvis; and thrombus or hæmatocele in the same parts. These causes do not invariably, or under all circumstances, produce fetor, but they all frequently do so; and, it may be added that tents and pessaries often rapidly induce fetor of the discharges, as well as anything introduced from without that will decompose or lead to retention of matters that readily undergo this change.

Extraordinary cases of retention of the entire ovum, at or near the full time, and cases of retention of the placenta, or of portions of it, are well known. The latter class of cases has been recently made the subject of a monograph by M. Hegar. Those to which I now attract attention are more recondite, and less familiar to the profession. It is well-known to obstetricians that part of an ovum may be retained and cause putrid discharges, which are sometimes accompanied with danger to life, and which generally disappear in a shorter or a longer time; but the following cases have points of interest which distinguish them from ordinary accidents, and give them interest and importance.

Writing on the subject of abortion, in his work on Midwifery, Dr. Burns (tenth edition, p. 334) says—"While part of the ovum is left, or the whole of the secundines are retained for a considerable time, we have another danger besides hemorrhage; for, within a few days putrefaction comes on, and much irritation is given to the system, until the fetid substance is expelled. Sometimes, if gestation have not been far advanced, or the piece which is left be not very large, it continues to come away in small bits for many months, during the whole of which time the woman is languid, hysterical, and subject to irregular sanguineous discharges, or often to obstruction. But more frequently the symptoms are very acute; we have loss of appetite; prostration of strength; tumid or tender belly; frequent, small, and sharp pulse; hot and parched state of the skin of the hands and feet; nocturnal sweats, and various hysterical symptoms. The discharge from the vagina is abominably fetid, and hemorrhage sometimes occurs to a violent agree."

The cases to be recorded illustrate consequences of imperfect abortion to which Dr. Burns does not make any reference. They show that part of an early ovum may be retained without any urgent symptoms for several months. Like Dr. Burns, Dr. Ramsbotham (Obstetric Medicine and Surgery, third edition, p. 666) believes that retained parts of placenta begin to decompose and be putrid very early, and at farthest within a period of some days; Dr. Ramsbotham, indeed, considers it worthy of remark that he has "known a number of instances in which the placenta was not thrown off for many days after the young embryon had passed, and yet it did not possess in any degree a putrid odour." The following cases show that months may elapse before the sign of putridity is observed. This lapse of time between the incomplete abortion and the supervention of serious symptoms, especially of putrid discharge, together with the general teaching on this subject, illustrated from the justly popular works of Burns and Ramsbotham, sufficiently account for the circumstance that the most interesting of the cases now recorded were inaccurately diagnosed by the physicians under whose care they at first happened to be.

In the title to this short paper I have written "Fetid Uterine Discharges." But the absence of fetor in some examples of imperfectly completed abortion is very remarkable, and almost unaccountable; and this absence of fetor renders such cases less likely to be correctly diagnosed and treated, and makes them, in this respect, of much importance. The presence of fetor, indeed, ought soon to attract attention, and lead to careful investigation of the case in which it occurs; but, in its absence, there may be no special symptom to lead the practitioner to suspect that the case under his care is one having any character to distinguish it from more ordinary uterine ailments.

The following case of retention of an ovum for seven months is a remarkable instance of the absence of fetor, or, at least, of its presence to so slight an extent, in a scrupulously cleanly lady, as never to be noticed by herself or by her husband, himself a physician. I would, indeed, have it understood that by absence of fetor I mean its absolute absence, or its being such as not to attract the attention of careful, cleanly individuals.

Case 1.—Mrs. —— was delivered of her second child fourteen months before consulting me. When the child was about seven months old she believed herself to be again pregnant, and therefore gave up suckling. But she soon resigned the notion of her being pregnant, all symptoms of that condition gradually disappearing. After this time her uterine health became impaired. She suffered from weight in the uterine region, slight bearing down, occasional irritability of bladder, irregular action of the bowels, occasional disorder of the stomach, and even sickness. She soon began to have a constant leucorrhœa of a brownish colour, and bright bloody discharges were never absent above a week, the loss in this way being occasionally somewhat profuse. Having these symptoms, she made the long journey from India to Edinburgh. Soon after her arrival here a severe metrorrhagia occurred, and I was called to see her. The uterus was found enlarged, its cavity being also larger than natural. The cervix was healthy, but preternaturally open, the end of the finger being easily passed just within the circle of the os externum. The case was diagnosed as one of retention of some of the remains of an ovum which had produced for a time the symptoms of pregnancy, had ceased to live, but had never been entirely expelled. A sponge tent was inserted into the cervix. On the day following its use both the tent and the blanched ovum were found in the vagina. They were very fetid, as was to be expected from the use of the tent. The ovum was a placental mass, an inch and a-half broad and above half an inch thick. On opening the bag of membranes a few drops of dirty brownish fluid escaped; no remains of an embryo or cord were discoverable, and the ovum perhaps never contained any, being addle from the beginning. The fœtal surface of the placental mass presented prominences distended with bloody fluid, such as are often seen in similar circumstances. The patient soon recovered, and after aborting twice, bore a healthy child at the full time.

Case 2.—A case somewhat similar to the preceding occurred to me several years ago. The patient was a soldier's wife who had miscarried at about the end of the third month of pregnancy. She was attended by a woman who told her that all had come away right. But from that time till I saw her, ten weeks after the abortion, she never ceased to have bloody discharge. At this time the placenta came away. It was not fetid, but had undergone peculiar changes which I do not here describe; only it was œdematous, and its fœtal surface was covered by numerous distended bladders of chorion of the size of half a boy's marble, and filled with bloody fluid.

The case which I have next to record differs from the two former in the circumstance of the presence of a very profuse and intensely fetid brownish discharge evidently containing pus, and in the discovery of decomposing structures. Besides, it was erroneously diagnosed and treated as a case of ulceration of the uterus. The speculum and caustic had been diligently used, this treatment being the resort, in all uterine cases, of many ignorant, and not a few careless practitioners. On this I say no more at present, because it would be out of place to enter now on the many gross abuses of a plan of treatment sometimes found very useful.

It should be remarked that in Cases 3 and 5 fetor did not commence till after the disease had lasted for a long time. I am unable to give a quite satisfactory reason for the delay of the appearance of the putrid odour; but the long delay in two of the following cases brings the two preceding cases into the same category with them, it being almost certain that in them also, had the retained structures not been completely discharged, intense fetor would soon have made its appearance. The following appears the most probable method of accounting for the delayed putrescence (as in Case 5): The ovum dying, or by some morbid process becoming disconnected from the healthy decidua, may pass away entire and without decidual structures, or break down and be discharged; or it may be accompanied by only a portion of the decidua. In such cases the remaining decidua may be supposed to retain its uterine connexions for an indefinite time, say weeks or months, and then being thrown off, may be retained in utero, and at last become putrid. When the cause of abortion is disease of the decidua, especially if there be hemorrhage beneath it, or when uterine contraction or spasm is strong, then persistent adhesion and retention will be evidently less likely to occur than when the decidua is healthy and uterine contractions are inconsiderable. This mode of explaining the delay of putrescence is scarcely applicable to instances like Case 1; for in such it is not decidual but ovuline, and especially placental structures, that are not putrid. And in such cases, while a prolonged vegetative life can scarcely be imagined to continue without growth or hydatigenous or other degeneration, yet it is easy to suggest that a slow decomposition without putridity may explain the phenomenon.

Case 3.—Mrs. B., æt. twenty-five, was mother of four children, the youngest of whom was born nine months before her admission into the Infirmary. After its birth she was for three days much troubled with after-pains, but made, upon the whole, a good recovery. A month after her delivery she began to have severe pain in the lower part of the belly, and in the back and thighs, with bearing down, and at the same time a leucorrhœal discharge made its appearance. She was so ill as to require to be constantly in bed or on a sofa. Four months after her delivery she menstruated, and this change thereafter returned nearly regularly. At these times the loss of blood was so severe that her physician plugged the vagina each time, and the discharge lasted long—on one occasion for nearly three weeks. Two months before admission the discharge became more brown in colour and extremely fetid.

When admitted into the Royal Infirmary she had the symptoms above mentioned. The discharge was profuse, brownish, purulent, and very fetid. The uterus itself was not remarkably tender, mobile, and without surrounding induration. It was much enlarged and capacious, its cavity being large and having a length of about three and a half inches. The cervix uteri had a healthy surface, but the os was unnaturally patent.

Sponge-tents were used to effect dilatation of the cervix, and the interior of the uterus was explored with a long polypus-forceps. Shreds of membrane and little masses of decomposing tissue were brought away, but it was found to be impossible to identify the special structure. The uterus was now repeatedly washed out by injecting freely a stream of warm water through a double catheter, such as is used in injecting the bladder. Numerous shreds of tissue came away. Ten days after this treatment was commenced the fetid discharge had disappeared;

the uterus was reduced to its natural dimensions, and the woman soon left the hospital quite well.

Even in women carefully delivered at the full time, accidents may happen analogous to those illustrated in the cases already mentioned. In most such cases, when a portion of placenta, or of membrane, or of both, is left adherent to the uterine wall, separation and extrusion or decomposition and discharge of the abnormally persistent parts is effected soon after delivery,—that is, within four weeks. I do not discuss the question of resorption of such parts, believing that such an event is, so far as fœtal structures are concerned, physiologically impossible. But there is no doubt that parts of the placenta may be retained undecomposed for a considerable period and then break down into fetid discharge, or be expelled entire. The following case will illustrate this to some extent; it is interesting because an able and careful practitioner and a good monthly nurse both satisfied themselves at the time of the confinement that the after-birth was delivered entire and not disrupted. There is a very important class of cases of menorrhagia or periodical monthly floodings occurring after delivery, from which this description of case is easily distinguished and separated by the two circumstances, that the cause of the flooding was a retained lump of placenta, and that the flooding took place only once, the cause of the affection being removed.

Case 4.—Mrs. P., multipara, was easily delivered of a child at the full time. After its birth some hemorrhage flowed, but it was arrested on the removal of the after-birth. She made a slow but good recovery; and the discharges were reduced to a very insignificant amount. Four weeks after delivery, on attempting to get up, hemorrhage took place, and on making a similar attempt next day blood flowed still more abundantly. Arriving at the bedside, I made an examination, and discovered a firm mass dilating the upper part of the cervix uteri. This I proceeded to seize with a forceps, intending to remove it by any means that might be necessary. It was easily brought away, and found to be a placental mass of the size of a large walnut. It was not in the least degree fetid. After this the hemorrhage ceased and convalescence was soon perfect.

It is generally believed that putridity always occurs very soon in cases of retention of a portion of the fully developed placenta. Dr. Ramsbotham remarks "that the placenta of an early ovum does not always putrefy when left in the uterus, although that of a child at full time, under the same circumstances, invariably takes on the putrefactive process, and that within thirty or forty hours." Case 4, just related, sufficiently proves the possibility of entire absence of putridity for a whole month.

But the most common and easily accounted for cause of fetid discharges from imperfect abortion, is retention of masses of decidual membrane. This can only occur in the early months of pregnancy; for, as this state advances beyond the third month, the decidua is rapidly thinned and atrophied as the uterus is distended, and does not anywhere present any mass.

The advancement of the pathology of abortion, a subject little understood, will surely throw much light on the subject-matter of this paper. At present I merely remark, that in abortion it sometimes happens that the entire double layer of decidua is discharged with the ovum: in this case the abortion may be truly called complete. It also sometimes happens that the ovum alone is discharged unaccompanied by any decidual structures; and in such cases this incomplete abortion is followed, after a few hours, or even a day or two, by the expulsion of the remaining decidual masses. Occasionally no such decidual masses are discharged as masses, and yet recovery is undisturbed; and in cases of this kind the persistent decidual membrane must either disintegrate rapidly and come away imperceptibly in the discharges, or, maintaining its uterine connexions, the membranes may slowly exfoliate and atrophy in like manner as it does after ordinary menstruation. But it also, though rarely, happens, that the decidual masses are retained for many weeks undecomposed [perhaps adherent to the uterus], and then become separated, putrefy, and cause fetid discharges, until they are expelled. In the following case, this series of events occurred; and it is to be noted that fetor was not observed till five months after the end of the first part of the miscarriage.

Case 5.—Mrs. M., æt. thirty-six, had borne several children. Her youngest child was six years old. Six months before admission to the hospital, she miscarried at about the end of the third month of pregnancy. In about a fortnight thereafter she felt quite well, having only a slight yellowish discharge. But, beginning to walk, she found the discharge become bloody, and it continued so, more or less, till she came to the Infirmary. In the fifth month after the miscarriage, the discharge became much more profuse, occasionally coming away in clots. Before the expulsion of clots she had always much abdominal pain. Shortly after this the discharge became fetid, and the stench about her person was sometimes so great that her children could not abide in the same room with her.

On examination there was observed a very fetid, grumous, bloody, and purulent discharge, containing some shreds of animal tissue. In the vagina was found a rounded decomposing mass of decidua of the size of an almond. Its rounded shape differed from that of the irregular lacerated piece it had originally been, just as a water-worn boulder differs from an unaltered angular fragment. Another piece of decidua like the former was soon afterwards discharged. The os uteri was morbidly patent, and the uterine cavity was three-fourths of an inch longer than is normal. The woman was in a very bad and weak state of health generally.

As fetid discharge still continued, the os uteri was dilated and the uterine cavity washed out with warm water, as was done in Case 3. In three days thereafter the fetor was no longer observed. The discharge rapidly diminished; the general health improved; and the woman soon left the hospital cured.

It is to be remarked that all the cases here recorded were characterized by more or less of the local pains of uterine ailment, by the presence of discharges which were either purulent or bloody, by occasional metrorrhagia, by patency of the os uteri, and a hypertrophied condition or a state of imperfect involution of the uterus. Besides, after the lapse of a considerable time, in Cases 3 and 5, the discharges assumed an intense fetor.

The importance of this class of cases need not be insisted on. By means, the use of which is illustrated in Cases 1, 3, and 5, the diagnosis will be easily made if the practitioner only suspects the true nature of the case, and is thus induced to investigate it.

For the cure, nothing is required but the complete evacuation of the offending structures from the interior of the uterus. This is effected by the use of the sponge-tent, the polypus-forceps, and the double catheter injection apparatus.

In conclusion, I wish to make a single remark on the pathology of abortion, with the view of suggesting inquiries which will certainly, when completed, throw much light on the subject of this article and on abortion generally.

In early abortions there is frequently observed great hypertrophy of the decidua. This hypertrophy of the decidua may be of two very different kinds. In the first, it is observed to be out of all proportion to the ovum and not otherwise notably morbid. In such cases the history often makes it evident that the ovum being addle or otherwise diseased had ceased to grow but has retained its vitality, while the decidua has continued to grow, and of course the uterus with it. Specimens of abortions of this kind are not very rare. The woman, often a person of experience, reports herself as six weeks or two months, or even farther, advanced in pregnancy; she aborts and discharges an immense mass of decidua having an ovum in one corner of it not bigger than a hazel nut. In the second kind, the ovum has grown to a size proportionate to the presumed stage of pregnancy, but the decidua is larger than is natural at that stage. In cases of both kinds, but especially of the latter, I have observed morbid conditions of the decidua, the result of sanguineous congestion and of inflammation: in some instances I have seen even extensive croupous exudation on the surface of the decidua.

I have already remarked that the ovum is sometimes discharged without any investment of decidua, and have pointed out what may be the future history of the persistent membrane. In connexion with this matter it would be interesting to find the true history of cases of discharge by abortion of one ovum in an early plural pregnancy which was not farther disturbed; whether it came away without accompanying parietal decidua or with more or less of it.—*Edinburgh Med. Journal, Jan.* 1863, *p.* 589.

102.—HYPERTROPHIC ELONGATION OF THE CERVIX UTERI.

Case under the care of Dr. Robert Barnes, at the London Hospital.

Failure of Cauterisation.—Amputation by Marion Sims' method—successful result.—G., a woman aged thirty-five, was admitted an out-patient on February 12th, 1862. She has had five children and one abortion. The last labour was two years ago; it was a cross-birth. Since then she has suffered much from metrorrhagia, dysuria, painful defecation, leucorrhœa, pelvic and dorsal pains, loss of strength and flesh, and bearing down. The os uteri was near the vulva, but there was no marked descent of the body of the uterus; the uterine sound penetrated more than three inches; the apparent prolapsus was due to the greatly hypertrophied and elongated condition of the cervix. The anterior lip was especially affected; it expanded into a hard tumour, which fretted the vagina by constant pressure and friction. The potassa cum calce was applied to the anterior lip, so as to produce a large eschar, and it was thought that some diminution of bulk followed the healing of the sore. The cauterisation was repeated several times in the course of the ensuing four months. The ultimate advantage was very doubtful. She then wore a stem pessary for some time, but this only doubled up the elongated cervix, without producing any diminution of bulk, or bringing the patient material relief. Dr. Barnes, therefore, determined upon amputation as the only means of restoring the patient's health.

On September 5th, she was admitted as an in-patient. Under the influence of prolapsus and friction, the enormously enlarged cervical portion, acting as a polypus or foreign body would, had caused considerable inflammation of the vaginal mucous membrane, as well as of that covering the cervix. Dr. Barnes kept her in bed some days, to allow this condition to be relieved under rest. This being accomplished, and a menstrual period having passed by, Dr. Barnes proceeded, on September 27th, to amputate the vaginal portion of the cervix on the method practised by Dr. Marion Sims. The patient being placed under chloroform, the uterus was pressed downwards by firm pressure over the abdomen, whilst the cervix was seized and drawn forwards by Museux's vulsellum, the vagina being opened by a duck-bill speculum on either side. The anterior and posterior lips of the hypertrophied cervix were then successively cut off by bent scissors. An even incised surface was secured by paring off the mucous membrane at the angles of union of the lips. Very little bleeding occurred. A small roll of lint, soaked in solution of perchloride of iron, was placed in the canal of the cervix. The stump was then covered by bringing together the edges of the mucous membrane by iron wire carried by the curved tubular needle. In this manner a very neat and perfectly covered stump was made.

No examination of the patient was made for ten days. At the end of that time, the greater part of the stump was found to have healed. The sutures were removed at the end of fourteen days. There was a little healthy suppuration from the central part, but the cure was essentially complete. The patient suffered very little; she passed a menstrual period without discomfort, was free from vaginal distress, and was made an out-patient on October 21. In November, having returned to laborious avocations, she experienced some degree of prolapsus which might be anticipated from the long-maintained relaxation of the pelvic structures. In general health she was quite restored. What little local distress remained was effectually relieved by the use of the stem-pessary generally adopted by Dr. Barnes in this hospital. There is now no excess of cervix in the vagina, and the os uteri may be considered normal.—*Med. Times and Gazette, Jan.* 3, 1863, *p.* 7.

103.—HYSTERICAL PARAPLEGIA FOR TWELVE MONTHS; GOOD EFFECTS OF ELECTRO-GALVANISM AND FRICTIONAL ELECTRICITY.

Case under the care of Dr. GULL, at Guy's Hospital.

Two very important manifestations of cerebro-spinal disease were simulated by hysteria in the following cases—namely, paraplegia and hemiplegia. The distinctive differences between the hysteria and the graver malady will be apparent from the history and progress of each case. Very good results were obtained from electro-galvanism in the first case, the notes of which were taken by Mr. W. Frank Smith, clinical clerk.

Matilda B., aged seventeen, living in Shoreditch, was admitted January 29th, 1862. She has no brothers or sisters. At the time of the accession of the symptoms with which the paraplegia commenced, she lived at home without any fixed employment. She had had, she said, very good health, but had not menstruated up to the present time. There is no evidence of her having been exposed to wet and cold; but about a year before the paraplegia she fell down a flight of stairs and bruised her back. The accident, however, did not prevent her going about as usual next day. Twelve months before admission the patient became suddenly insensible while lying awake in bed, about nine o'clock at night, and was found in this condition by her mother. The absolute unconsciousness lasted about two hours, and during that time the mother supposed her dead. On returning to consciousness, she found she could not move her arms or her legs, and had lost sensation over her whole body up to the neck. She has regained feeling and motion in her arms, so that she can work with her needle. She has never lost power over her sphincters. She is a stout, well-nourished girl, with coarse and heavy features; pulse 80, weak and regular. The power of motion is impaired equally in each leg, which she can raise to the extent of two or three inches. Loss of sensation is well marked from the feet to the upper thirds of the thighs; within these limits a severe prick is unfelt. Above this, sensibility increases up to the hips, where it is normal. The urine is natural.

Jan. 31st. The passage of the electro-magnetic fluid along the course of the extensor muscles of the leg produced contractions of the muscles when the intensity of the current was great, and she could feel the effects; but though the current was increased to its fullest strength, it seemed to cause her no pain.

Next day, with the assistance of the nurse, she managed to walk as far as the electrifying room, dragging both legs after her as she went along. Frictional electricity was employed along the spine. The arms were as sensitive to the influence of the coil-machine as in a healthy person, but not so the legs.

Feb. 5. The catamenia appeared last night for the first time, and continued quite normal to the 7th, when sensation became normal everywhere, but she could not stand alone.

10th. Frictional electricity was applied along the spine for fifteen minutes, so as to produce slight redness. This was repeated on the 14th.

From this time the patient remained in the ward as much a helper as a patient. She was not electrified for the next two weeks in consequence of the machine being broken.

March 3rd. She assists in scrubbing the floors of the ward; but she still walks with great difficulty, dragging her legs after her. The frictional electricity was resumed to-day.

6th. She felt very miserable this morning on rising, and took to her bed; complains of headache; is rather hot and feverish; pulse 84.

26th. Continues much in the same state; the catamenia did not recur; the bowels were kept open by hellebore purgatives daily, and electricity was regularly used.

27th. Pain in the cardiac region; faintness and loss of appetite since yesterday. The heart sounds are quite clear and normal. It is now seven weeks since she menstruated.

April 20th. Much the same as during the last four weeks,—*i.e.*, with improved powers of walking.

May 14th. She seems almost well; but she will not admit it, nor will she do any work whatever. She can walk quite well, and was discharged.—*Lancet*, *Jan.* 17, 1863, *p.* 62.

104.—A MODEL HYSTERIC.

By Robert Ellis, Esq., Obstetric Surgeon to the Chelsea and Belgrave Dispensary.

I have at length succeeded in permanently curing one of the most inveterate and obstinate cases of hysteria which I have ever known, or of which I have ever seen a record. As the lady has presented me with copious notes as to her previous history, which offers many points of interest, and some of practical value, I venture, with her consent, to offer a sketch of the case.

Miss A. B., an unmarried lady, in easy circumstances, in the winter of 1829 first became the victim of a disorder which, true to its portean character, assuming many aspects, never wholly left her for thirty years subsequently. She was at that time an excitable and impressible girl of eighteen; but I have been unable to discover any distinct emotional cause for the development of her hysteric disposition. She had a cough and pain in the right side, which was treated in the manner customary at that period,—by the repeated application of leeches. In the following spring she was taken to Dr. Abercrombie, who advised leeches again, followed by a blister, and this was repeated for several months, the purely hysteric element in her case never having been recognised. In 1831 she became a patient of the late Sir H. Marsh, who, recognising the real type of her disorder, prescribed, with a temporary relief, shower-baths and fresh air. But, her pain becoming very severe as winter approached, she again returned to Dublin, and this time was cupped on the side. From that moment a new symptom made its appearance, in complete paralysis of the bladder. This was at first overcome by injections of turpentine and castor-oil and warm hip-baths. But in a short time these remedies lost all influence, and she was allowed to remain with the bladder unrelieved for thirty or forty hours. Sir Philip Crampton, who then saw her, and was apparently perplexed by her state, directed the catheter to be used twice daily. This, however, aggravated her distress from the excessive irritability of the urethra, to relieve which large doses of laudanum were administered. She was now blistered on each side of the spine, and occasionally over the pubes, with but little good result. Issues were opened at intervals down the spine, and kept discharging for a considerable time. Thus the unhappy sufferer dragged on a painful life until 1834, when the hysteric cough came back, and was again attacked by repeated blisters. She had now fully learned the soothing influence of morphia, and was supplied with as much as she required. The disorder again returned to the bladder, and was this time overcome in a rational manner by the use of enemata of starch and opium into the rectum. So effectual was this remedy, that for many months the disorder did not recur; but the cough returned with its disappearance, and probably would have continued but for an intercurrent attack of diarrhœa, which left her quite free from all hysteric symptoms for several months.

In 1836, great domestic sorrow and anxiety prepared the way for a return of the old disorders. The bladder became as helpless as ever, and the old treatment of escharotics and issues to the spine was had recourse to. The hideous seams and numerous white cicatrices still visible on the back of this unhappy lady would almost suggest her having passed through some sanguinary field of battle. It was perhaps unfortunate for her that this violent treatment afforded a temporary relief, since it was persisted in to a most formidable degree. Suddenly the bladder acted as well as if nothing had happened, and now the patient got into a fresh trouble. She was taking pills with a minute quantity of strychnia. "I do not know whether the pills did me good or harm, but about this time I got a trick of falling down when I attempted to walk, and after a laudanum injection; but by keeping quite quiet came all right again." The hysteric paralysis had seized the voluntary muscles this time. Headaches and confusion of intellect now occurred, and offered a new and tempting opportunity for vigorous treatment. She was dry cupped again and again on the nape of the neck, and as soon as the glasses were removed a seton was inserted. Her dose of laudanum now reached two drachms.

In this state she became a patient of an eminent physician at Leamington. There was now, instead of dysuria, a constant flow of pale urine. Her treatment commenced with calomel in cold gruel, and the laudanum was stopped. She became much reduced, and all her distresses were aggravated, especially at the menstrual periods. A tonic treatment was attended with a better result, and in a few months she became for a time tolerably well. Occasionally, however, the palsy of the bladder would recur, and was relieved by a dose of Jeremie's solution of opium. The diarrhœa seemed to take its place, and I think there can be little doubt that this symptom had a purely hysteric origin. The old seductive influence of the opiates became again felt, and they were freely used.

In May, 1843, the bladder again giving trouble, suddenly, and without any previous warning, a severe attack of trismus ensued. The first invasion of this new malady was short, lasting only an hour or two at a time, and *invariably induced by irritation of the bladder.* Soon, however, it fully established its claim to a place in the chain of her morbid tendencies. The jaws would continue closed for a day, then for days, and at last even for months, with intervals of respite. In one of these the eyes suddenly closed and remained shut for a whole month! resisting all treatment and efforts to open them. During this period, the hysteric irritability being apparently at its height, she lost the use of every limb, and was perfectly helpless. The shock of being roughly carried down stairs and into a new house broke this spell, and in a tempest of hysterical passion she opened her eyes, and they only closed again for short periods. The trismus, however, would not let her go, and one long fit lasted for nine months, terminating, as usual, suddenly. She was taking much opium all this time. Sometimes she appeared comatose for eighteen hours, and then woke with an open mouth. If the trismus had lasted for days it was always at an end immediately on the occurrence of a fit of hysterics. Sometimes it yielded (as, indeed, might have been expected) on the sudden application of scalding water to the cheek.

Down to 1850 the state of this lady remained with little alteration, the trismus and the urinary difficulty only occasionally abating, and never wholly leaving her. In that year, during a period of home trial, the trismus relaxed, and was replaced by a violent spasm of the muscles on one side of the face, distorting the countenance in the most horrible manner, and entirely preventing her swallowing even liquids. The inhalation of chloroform was then commenced, and proved effectual in subduing this spasm for a time. This new malady held its ground for a long time, and the chloroform became a necessity in order to enable her to take her meals. An attempt was then made to bring her under the influence of mercury, and she had the advantage of a scruple of calomel for a dose once a week! but no salivation ensued—in all probability the bowels and kidneys carrying out the drug. Profuse salivation was, however, at last the reward of a persevering inunction of mercurial ointment, with smaller doses of calomel often repeated. The spasm and trismus then vanished for two months.

An investigation into the state of the uterus was now made, and it was discovered that a small polypoid growth was hanging from the cervix. This was twisted off, but no change in her state ensued. She became rather worse, and used up at least a pound of chloroform every three weeks! Thus the case continued down to 1855, to the dismay of all who attended her. In the spring of that year a mimic paralysis agitans appeared on the scene, occurring with marked violence at the return of each menstrual period. In this state of hysteric oscillation, from the semblance of one disease to that of another, she came under my care. This was in the autumn of 1858.

The chloroform bottle, the opiate, and the catheter were now indispensable to her. She was sallow, haggard in expression, nervous and emotional in the highest degree. She still menstruated with great regularity, and her trismus and inability to empty the bladder remained as bad as ever. The teeth of the upper jaw, from constant pressure on the corresponding teeth below, were worn down in a remarkable degree, and the masseter muscles on each side had undergone a degree of hypertrophy very significant of their constant exertion during so long a period. A careful examination into her case, removed from it, with two exceptions, every trace of organic disease. The uterus was unhealthy. There was much congestion and some enlargement of the cervix and from its posterior lip hung down a small polypus, half the length of the little finger. There was in addition an unhealthy bunch of inflamed piles. After removing the polypus, and by repeated applications of nitrate of silver, and once or twice of stronger escharotics, the cervix was got into a perfectly healthy state. During this time a great amelioration took place in her state of general health. We had agreed to put the chloroform and the opiate on the shelf, and the catheter was also locked up. I then removed the piles, some by excision and some by ligature; "and from this," writes the patient, "I rose up in about ten days more free from pain than I had been for years." She then went into the country, and in a few months gained so much in strength and flesh as to be a wonder to herself and others—chloroform, opium, and the catheter were all disused and nearly forgotten.

It was certainly a triumph over the hysteria, but, as it proved, it was not of very long duration. The menstrual crisis was now approaching, and we were to have one more long struggle with this obstinate, often beaten, but unconquered foe. The monthly discharge beginning to occur irregularly, was at last attended on one occasion with much pain and discomfort and a feeling of bearing down, in fact a state of physiological congestion without relief by the usual flux. The old irritation appeared to leap back into its accustomed seat, and in April, 1859, this poor lady presented herself again as bad as ever, and, I am sorry to add, the opium had resumed its accustomed place on her dressing-table. I once or twice applied two or three leeches to relieve the uterine congestion, and always with benefit to her other symptoms, but as it was obvious this could not be repeated constantly, it became desirable, if possible, to overcome the hysteric disorder in some other way.

As there appeared to be some irritation in the canal of the urethra, relief was given to that by smearing the catheter with a little extract of belladonna. Hypodermic injections of morphia and other anodyne substances into the arm were then used, and invariably opened the mouth in a few minutes; occasionally also chloroform was given. In fact, the old routine was being gradually reopened, during the time I was awaiting the completion of one of Legendre and Morin's instruments for applying the electricity of induction, which Duchesne has called after the name of its discoverer Faraday. Upon the application of this remedial agent (the only means as yet untried) my hopes of success were grounded, and, as the result showed, not without good cause.

I resolved to overcome the spasm of the muscles closing the jaw by throwing into action those whose functions it is to open it, and which, in comparison with the former, were feeble and ill-nourished. Also it was determined to arouse the muscular contractility of the bladder, weakened by long abeyance of its activity and by the distension of its walls from long retention of urine. As the patient had been saturated with nervine tonics of every description for many years, she was to take nothing but a little tincture of sumbul in camphor-water occasionally.

The first application of the Faradising instrument to the depressors of the jaw was perfectly successful. The opposing muscles, oddly though it may sound, appeared taken by surprise, and yielded in a very few minutes with a sort of snap. I gave the patient a pretty strong dose of the electric current, and she was informed that it would continue to increase in its force every day until the mouth ceased to shut! Then with a moistened sponge on the conductors, the abdominal muscles were first acted upon. They were in a state of hysteric rigidity, and no doubt thus helped on the seeming paralysis of the bladder. On the right side there was much less contractility of these muscles—and, indeed, generally of the muscles of the body—than on the left, and this was developed in a striking manner by the varying tolerance of the electric current. There was also a most marked anæsthesia of the skin all over the right side of the body. By soldering on a small brass button of the size of a small pea to the end of a piece of brass wire, and then enclosing this in an elastic catheter, I manufactured an excellent instrument for carrying the electric current to the walls of the bladder itself without communicating it to the urethra, which would have been intolerably painful. By the moistened sponge conductor applied over the pubis, and by connecting the wire of the catheter with the other pole of the instrument, we succeeded in arousing the torpid muscular irritability of the bladder. This was the treatment on which I based my hopes of an ultimate victory over the bladder, and it proved in the end quite successful. At the same time, having removed the irritability of the urethra, I determined to overcome temporarily its contractility, by gently dilating it as in the removal of stone from the female bladder, and thus break the habit of retention of urine. This was effected under chloroform by Weiss's dilator, and excellent results ensued.

In a short time an obvious improvement took place in the patient's general state, and in her increased muscular powers. The depressors of the jaw acquired strength and a fuller increase of volume, and there was a very marked increase in the muscular power of the bladder, so that at last on using the catheter it projected a forcible stream, instead of the urine coming away in a mere passive flow. Occasionally a parenthesis of hysteric spasm would occur in the muscles of the face or of the neck. One morning my patient came to me with a mimic torticollis, and another time with her face drawn to one side in the most grotesque fashion. In the manner of a very decided rebuke for such unreasonable conduct, she received on each of these occasions so strong a dose of the electric current as to put an end to the difficulty as if by magic! We were thus gradually dislodging this martyrising disorder, by, at the same time, giving her the muscular power to vanquish it, and calling out the will to that end. Much patience and a most determined resolution to conquer—at first wholly on my part, but afterwards very decidedly on that of the sufferer—brought this strange case to its close. The last thing done for her was on Saturday the 9th of June, 1860, and on Monday, 11th, she came, overflowing with gratitude, to announce herself set free from her disorder.

It is now nearly two years since this lady was discharged cured of her hysteria, and from that time to the present not even the shadow of any of her former annoyances has fallen over her history. Considering the long duration of her disorder—more than thirty years—its extraordinary pertinacity and obstinate recurrence, we may fairly conclude two years' absolute immunity to represent a complete cure. And as the menstrual function is now closed, and a source of irritation thus removed, we may fairly hope that this lady, who was called by some of the eminent physicians who attended her the "Queen of Hysteria," has vacated her throne to some younger successor.—*Med. Times and Gazette, April* 4, 1863, *p.* 341.

105.—ERGOT OF RYE IN MENORRHAGIA.

By Dr. GRAILY HEWITT, Physician to the British Lying-in Hospital.

[In many cases of menorrhagia a treatment conducted on general principles is for the most part successful: we improve the hygienic condition of the patient, and make such alterations in her habits or mode of life as appear to be necessary. When there is actual disease of the uterus, or polypus is present, the indications for treatment are different. In some cases, however, the patient is the subject of a profuse loss of blood, the sole discoverable alteration of the uterus being a lax, uncontracted, atonic state of the organ; and a gentle but continuous oozing of blood is taking place, for the arrest of which we require a therapeutic agent quick and certain in its action, and which is capable of exciting the uterine tissue to contract.]

Such an agent we have in the ergot of rye. After making trial of its effects in several cases of the kind above alluded to, and having compared the results with those obtained after the use of other remedies, and especially of digitalis, I have come to the conclusion that the ergot is far preferable, and that in pure, uncomplicated cases of menorrhagia it is the best remedy in our possession.

The following case exemplifies, as fully as one case taken by itself can do, the good effects of the remedy in question:—

Mrs. M'G., aged forty-three, a rather stout woman, of good conformation, came under my care (acting for Dr. Tyler Smith) as an out patient of St. Mary's Hospital in October last. She stated that she was the mother of thirteen children, the youngest of whom was born a year and a half ago. Menstruation was quite regular up to six weeks ago. The discharge then came on as usual, and lasted for three days. During the night she was called up by a knocking at the door, and imprudently walked over the cold steps to the door, and back to her bed. The discharge was thereby suddenly arrested for a few hours, and then there occurred a very severe flooding. Ever since that time she has continued to lose blood every day, and during every hour of the day, the quantity lost being more or less great at different times. No effect has been produced by the treatment to which she has been subjected, and she is now excessively pallid and anæmic, and so weak that she is hardly able to walk. She complains also of a giddiness in the head, singing in the ears, and imperfect vision. On examination, I found blood oozing slowly from the uterus; the os rather soft and wide open, and the whole organ evidently in a very relaxed atonic condition. She was ordered to remain in bed; to apply cold to the hypogastric region; to take a little brandy-and-water and beef-tea occasionally; to drink lemonade, and to take night and morning one teaspoonful of ergot of rye in powder, mixed with a little boiling water. Three days later she sent to the hospital to say that she was much better; that the hemorrhage had been arrested after the first dose of the medicine, and she was desirous of knowing whether she was to continue it, as every dose "made her so sick." The patient had lost so much blood that it was evident further loss might be attended with danger to life, and it was hardly anticipated that so immediate an arrest of the bleeding, which had been going on for six weeks, would have been procured.

The action of the ergot in determining the cessation of the hemorrhage in these cases is probably dependent on the contraction of the uterine tissue to which its administration gives rise; it is, in fact, analogous to the action of ergot when given to arouse uterine action in cases of inertia of the uterus, hemorrhage after labour, &c. The condition of the uterus in a long-standing case of memorrhagia such as that above related is one almost completely identical with that of the uterus after an abortion; the walls are thick and full of blood, and the hemorrhage continues simply because there is an absence of any tendency to contract. That contraction is produced by the ergot. There does not appear to be any evidence that, apart from its contraction-inducing power, the ergot has any special hemostatic effect such as has been attributed to it. Some authors have objected to the above theory of the action of ergot in these cases, that in the unimpregnated uterus no contraction, properly speaking, can be said to take place. To this it is sufficient to reply, that if the presence of pains, sharp, short, and like miniature representations of the pains of labour, can be said to be a proof of the occurrence of uterine contractions, then we have proofs in sufficient number. One part of the action of ergot in these particular cases is to give rise to such pains—uterine colic, as they are appropriately termed by continental writers.

The fact that each dose of the ergot gave rise to vomiting in the case above related is worthy of notice. In a subsequent paper I propose to consider particularly the nature of the action of ergot in producing contraction of the uterus in purely obstetric cases, and shall then have occasion to refer specially to this element in the action of ergot—namely, the vomiting and nausea which frequently attend its administration.—*Lancet, Dec.* 13, 1862, *p.* 646.

106.—ON CAUSES OF EARLY MORTALITY.

By Dr. J. Whitehead.

I have long felt persuaded that childbearing, as a rule, is a much more precarious process among the educated and wealthy than among the untaught and ignorant classes. The equably-poised temperamental elements displayed in the healthy constitution of the contented peasant, unracked by mental strain—the *temperament tempéré*—is the happy condition best calculated to encounter shocks and vicissitudes with ease and safety. The system of the highly-educated female is a very different piece of machinery. Subjected from early childhood to an exhausting mental discipline, whatever may be the lack of capacity or inaptitude for it, and often regardless of the physical culture so necessary to the preservation of health, the vital organic tone becomes early depraved, and the nervous element either disproportionately developed, or rendered too morbidly sensitive to sustain the body with comfort under the exercises essential to its own well-being.

The following particulars bearing on this subject are extracted from the histories of 753 mothers of the educated and wealthy classes, all derived of parents similarly circumstanced as to cultivation and social position. Their ages range from 19 to 43, and all were, at the time of the enquiry, either bearing or nursing children. Their mean age was 30·5 years; the average number of their pregnancies, including that in some of them not yet completed, 4·83; the average of their abortions, 1·56, yielding a lower estimate of pregnancies, but a great preponderance of untimely events. The results for the two classes stand thus in contrast:—

	Average age.	Average of Pregnancies.	Average of Abortions.
Indigent class	30·36	5·20	·53
Wealthy . .	30·50	4·83	·56

The disparities here represented as existing between two classes of mothers very differently circumstanced, are, in my belief, by no means fortuitous or exceptional; a greater number of children being brought to perfection and reared, and fewer abortions experienced, by the untaught generally than by the educated.

The preceding statements have reference only to abortions which happen during the middle periods of pregnancy, when the various organs of the fœtal body, even at the earliest date of this range, are clearly defined, and the sex distinguishable. These, however, constitute but a small proportion of the total amount of this class of events. The death and voidance of the embryo at earlier periods is a much more common occurrence, though its actual frequency may be difficult to estimate. Were the so-called false conceptions, and the delayed and other catamenial irregularities, to be included in the category—as, indeed, they ought, being in most instances the result of, not false, but of real conceptions—the sum of abortions would probably equal, at least, that of successful pregnancies. A wife, of two or three or half-a-dozen years' date, applies for relief on account of inability to discharge her household duties with satisfaction, or to enjoy the comforts around her. Free from all tangible organic ailment, she is nevertheless an invalid. Healthy before marriage, she has not been well as a wife; she has no offspring. It is not uncommon with her friends, under these circumstances, to attribute her alleged indisposition to disappointment.

Such a person will probably state, that soon after marriage, her monthly visitations, regular and comfortable previously, have become changed in character or periodicity, and are attended with suffering. Sometimes delayed, occasionally too frequent, the product is unusual—fibrinous or membranous, broken, or grumous; now and then offensive; and each monthly crisis constitutes a paroxysm of serious illness. She loses flesh and strength, and mental elasticity, and is chided for being mopish.

Another, since marriage, has had the menstrual function delayed to the fifth or sixth week, or longer, when a hemorrhagic change took place, which is looked upon as an abortion, and probably was so; and for years afterwards she has periodical metrorrhagia or other form of irregularity, with distressful suffering, but no pregnancy,—or, at least, no offspring.

In other cases, to the birth and comfortable nurture of one or several children, succeeds one or more abortions, then menorrhagia, with morbid product and dysmenorrhœa, anæmic debility, and, sooner or later, some form of organic mischief—

commonly of the lungs or heart, and all without any clearly assignable first cause.

It is by no means uncommon to hear a woman, thus or similarly circumstanced, declare that she has miscarried six, or eight, or ten times in the space of one year; but her statement is seldom credited in this sense. The phenomena she describes are commonly classed under the vague denomination of dysmenorrhœa or hysteria, without reference to cause. It is not improbable, however, that the patient's own interpretation was the correct one; for, under the various forms of morbid irritability of the uterus and its annexes, conditions of daily frequency, that special and all-important process which nature sets up consentaneously with conception, for the purpose of forming and organising the primary envelope, being frustrated in the onset, the fertilised ovum, no adequate means being provided for its reception and accommodation, is thrown off soon after its arrival in the uterine cavity. There can be no room to doubt that accidents of this kind are much more frequent than is commonly supposed or believed.

Thus, the highest rate of mortality of the human product takes place, in all probability, during the tenderest state and earliest stage of its existence. It is scarcely questionable, indeed, that abortions are, by far, the most common during the first three months of pregnancy, and especially during the first third of this period; but, owing partly to the minuteness of the object voided, sometimes to its partial dissolution, or shrinking into a shapeless, shreddy mass, and, perhaps, also to the familiar frequency of the occurrence, the embryo is suffered to pass away unnoticed and unsought for.

After the third month and upwards to the sixth, such events are deemed of greater moment, because commonly attended with keener suffering, and not seldom with exhausting hemorrhages. The fœtus, moreover, too large to be shrivelled down or concealed by coagula, is easily detected. It is at these periods that such cases are more especially brought under the notice of the Medical Practitioner; while, at earlier stages, his aid is not solicited; or, if so, it is generally to remedy sequences, and at a time when every primary evidence of conception has passed away.

Of abortions during the second three months, that is to say, from the latter part of the third to the end of the sixth, the greatest number take place in the first, a smaller number in the second, and the fewest in the third portion of this term. Thus, of 452 such cases which occurred under my own notice at the St. Mary's Hospital, and published with others in 1847, there were 275 in the first, 147 in the second, and 30 in the third division of this period; standing for these respective epochs, in the co-ordinate relation of 9, 5, 1. Of more than 2000 similar cases since recorded, the results are nearly the same, the difference amounting only to a small fraction for each item, not sufficient to change the proportions above stated.

In the same space of time during which those 452 abortions were registered, there occurred, among the same class of patients, 115 premature births—32 in the seventh, 55 in the eighth, and 28 in the ninth month. Of these, 85 were still-born, and 30 living at birth, of whom only four survived to the end of one month.

Thus it will appear, that for every premature birth during the last three months of pregnancy, there are four abortions during the preceding three months. And taking it for granted that the number of such events during the first three months exceeds those of the second in the same ratio as the latter do those of the third—which is by no means improbable—then the relative frequency of immature births in the first, second, and third epochs of pregnancy of three months each, will stand in the progressive order of 16, 4, 1.

Supposing, further, that of the 43,752 children still-born in France in 1858, 20,000 were premature, which is doubtless quite within the actual estimate, the number of abortive births in that year, exclusive of the premature, would, according to the preceding mode of calculation, amount to the sum of 1,280,000, against 969,343 live-births, all included.

Should it prove, moreover, that in the aborted ova the preponderance of males over females, as compared with their relations in still-births, goes on to increase in the same ratio that the latter are known to do in comparison with those born alive, the number of males conceived would be more than double that of females. This, however, is scarcely probable.—*Med. Times and Gazette, Dec.* 20, 1862, *p.* 654.

MISCELLANEOUS SUBJECTS.

107.—ON THE TREATMENT OF SUSPENDED ANIMATION.

By Dr. Benjamin W. Richardson, M.A., Senior Physician to the Royal Infirmary for Diseases of the Chest.

[The writer includes under the head of suspended animation, only cases where life has been arrested by the suspension of that process of combination between oxygen and blood on which the calorification, and thereupon the animation, of the animal depends; whenever any serious mechanical injury has been inflicted, whenever the blood has been disorganized, wherever any important organ has been subjected to antecedent morbid change, the case is excluded. The cases included are very numerous: there are embraced cases of strangulation and drowning, cases of death from arrest of oxidation by the inhalation of carbonic acid, chloroform, amylene vapour and similar volatile compounds; cases where from shock, without structural injury, the chemistry of respiration has been suddenly arrested, including cases of simple stun from a fall or blow, blow on the stomach, lightning stroke; and, lastly, cases where oxygenation is suppressed by the indirect action of extreme cold. It is singular that there are great differences in the amount of muscular irritability after death from these various causes: the irritability remains longest after death from chloroform, and shortest after death from drowning.

Proceeding to the means to be employed for restoring animation, Dr. Richardson supposes a healthy man to lie before the reader, who has been subjected to one of the forms of death to which reference has been made. His breathing has been stopped; we listen to the heart, it is silent; the animal fire is declining: What shall we do?]

Suppose we inflate his chest with air?

Presuming that there is still a small column of blood extending from the right side of the heart to the left; presuming that there is some remaining action of the heart on both sides, we may by gentle insufflation fan that action into more perfect action, but by rude shock we may also fan it out altogether; artificial respiration has done both these acts; the credit that attaches to it rests with the first of these, the discredit is hidden in the last. If the column of blood from the right to the left side is broken the inflation of the chest is useless; as well blow a coalless fire-grate as bloodless lungs. Yet, as it may happen that the column of blood is not absolutely broken, nor the action of the heart actually stopped, even though the respiration shall have failed, artificial respiration does sometimes do strange things.

The application of artificial respiration becomes extremely limited: it is of use only where there is still some circulation; it must be secondary to circulation, because natural respiration is so.

Artificial respiration, if it fails at first, if it does not catch the column of blood which may be crossing the pulmonic circuit, instead of being a means of restoring life, clenches death.

Artificial respiration does not depend for success in any case on the amount of air which, by some particular plan, can be supplied by it; artificial respiration cannot at any time supplement the worst attempt at natural respiration; artificial respiration performed by any method that disturbs the body is injurious to the feebly-acting heart; artificial respiration, if it is to be of service, is amply provided for when by any method from ten to fifteen cubic inches of air can be gently driven or drawn into the chest.

Let me be saved, therefore, from hurting any inventor's feelings by an opinion on a point which is entirely secondary in its meaning. I prefer those simple double-acting bellows, first made by John Hunter, to anything and everything; they are portable, they are easily applied, they admit of graduation, and they are certain. If I wish to throw fifteen cubic inches of air into the chest, I can make sure of doing it, so I prefer the bellows for their certainty; but I am not prejudiced, and if in any given case I had no bellows, I should have recourse to the next readiest means—Dr. Silvester's method.

[Dr. Richardson has tried a great number of experiments with galvanism, very interesting, but too long for insertion here; we, however, give the general results.]

The results derivable from galvanism are negative in kind, but unhappily they are more than that. If they sometimes excite muscle to contraction, they also not infrequently paralyze it at once, while they always exhaust its power with great rapidity. If, in an animal just dead, galvanism be applied to muscle after muscle, each muscle may be paralyzed separately; all the muscles on one side of the body may thus be bereft of their irritability, while the muscles of the other side, which have not been galvanized, retain theirs.

These phenomena, applicable to all muscles, are specially applicable to the heart. The heart, of all organs, loses most quickly its response to galvanism, or responding to the current, is most quickly overpowered by it. These facts, first observed by Aldini, but not understood by him physiologically, I have confirmed by forty observations specially conducted.

I can stop the heart that is pulsating by the galvanic force, as readily as I can excite it by the same agent—aye, even more readily. I can wear out the excitability of the heart of one animal by gentle galvanism more quickly than the heart of another animal similarly placed shall wear out its own irritability by its own contractions. Hence galvanism is a delicate remedy as applied to the heart.

[Shall we transfuse? To obtain an answer to this important question Dr. Richardson has performed many experiments on animals, but comes to the conclusion that it is a proceeding which is "not of much promise." With regard to the advisability of trying artificial circulation, he observes:]

If any plan could be devised by which blood could be drawn over the pulmonic circuit without opening the blood-vessels, the difficulties of artificial respiration would be surmounted. Warmth to the surface, long continued, not intense, as in the hot-water bath, but gentle warmth—has this any power in the direction stated? It dilates the capillaries and the skin reddens, but is it ever sufficient? I confess that in animals I have seen effects so singular from long-continued warmth, that I do not know, if we could understand all its bearings, to what it might not lead.

Some kittens were drowned in my house, and lay at the bottom of the water for two hours. I took one of these out of the water, wrapped it in a portion of flannel oil-cloth, and put it in my pocket with the intention of taking it to lecture, to illustrate the foramen ovale. The day was hot, and I had some distance to go before reaching the lecture theatre. I thought nothing of the kitten till I wanted it; putting my hand into my pocket to find it, I was astounded to discover movement, and on taking it out, it was breathing; in the course of a few hours it recovered perfectly.

These occurrences, in the inferior animals, are not uncommon; in the London mews, I find the fact is generally known that animals drowned or strangled, and sent to the manure-heap for burial, often recover, even some hours after they have been placed there. Such an occurrence took place in a cat last year, in a livery-yard near my house.

Even while this paper was being completed, I received from

Dr. Black, of Chesterfield, a history of a similar phenomenon, which occurred at the house of Mr. Thorpe, of Stavely. I will give Dr. Black's account.

"A short time ago he (Mr. Thorpe) ordered his groom to destroy a kitten a few days old. The man first nipped its neck tightly for a short time, and afterwards threw it into a bucket of water, in which it was immersed *two* hours.

"He then, believing the kitten to be dead, buried it in a manure-heap, and naturally expected to hear nothing more of it. Two days afterwards, Mr. Thorpe was passing the manure-heap when his attention was arrested by the mewing of a kitten apparently proceeding from the heap. The groom was fetched and questioned upon the *death* and *burial* of the kitten. He stated the above-related facts, declared that the kitten was *quite dead* when he took it out of the water, and that he buried it in the manure-heap. He now uncovered the place whence the mew of a kitten proceeded, and found the one which he had buried there two days before, *living* and *able to walk!*"

Unfortunately, up to the present time, these isolated but valuable facts cannot be reduced to exact formulæ by experiment.

I have tried the exposure of animals to warmth in baths of different temperatures, in sand, in moist warm straw, in moist warm air, but have never seen anything approaching to spontaneous recovery. Yet it deserves mention that in one instance in a rabbit which was laid in sand at 100° Fahr., the muscular irritability of the intercostal muscles was present thirteen hours after death. It is also worthy of remark, that in all these cases, on inspecting the body, the right side of the heart was always found free of engorgement, and the left side and the arteries containing blood ; conditions indicating obviously the transit of a pulmonic current.

And now, to what summary do all these researches and observed phenomena lead ? Do they tell anything positive ? I will state what they tell.

1. They indicate that artificial respiration can prove of avail only while the heart is transmitting a pulmonic wave of blood, and that in the absence of this wave, artificial respiration is injurious ; injurious to the lungs and fatal to the blood.

2. They indicate that if a current of blood can be made to traverse the arterial channels, the muscles of respiration, previously at rest, will resume their action ; and that respiration will follow, as at birth.

3. They point out that the effect of gentle external warmth is to induce an arterial tide ; and that in every inquiry in respect

to re-animation, the most careful attention must be directed to the further elucidation of this one subject.

4. They point out that galvanism is a dangerous remedy in all cases; that its effects cannot be measured, and that while it may set up a temporary excitation, it wears out excitability.

But more than all, they indicate that between the time of so-called death and the period of the coagulation of the blood, re-animation is a possible fact; that the same rule, being applied to the body at large, as is often applied to the finger or other part of the body removed by accident—namely, the re-establishment through it of a blood-current—the body would re-live as a whole as it does in part; in a word, they show that the old principle, however it may at present fall short of demonstration, is true: "that which is of the local is of the general."—*British and Foreign Medico-Chirurgical Review, April*, 1863, *p.* 490.

108.—AN ACCOUNT OF THE PHYSIOLOGICAL RESEARCHES OF THE REV. PROFESSOR HAUGHTON, M.D., TRINITY COLLEGE, DUBLIN.

An elaborate series of experiments on the urine of healthy men was undertaken by the Rev. Professor Haughton, M.D., of Trinity College, Dublin, with the object of establishing a theory of work applied to man, considered as a machine, mechanical and mental. Reflection and observation suggested to him that the natural and proper measure of the value of a given diet was the amount of work it would produce in a healthy animal such as man. Bernouilli was of opinion that the total quantity of daily work of which a healthy man was capable was constant, no matter in what description of labour he was employed, but the state of knowledge in his time did not admit of the development of his idea; the facts, as he understood them, were against his view, and he was obliged to give up his opinion. The view which Professor Haughton takes of the subject is as follows:—A man in health consumes per day a certain amount of food and drink, and excretes per day an equivalent amount of waste matter, supposing him to be full grown, in good health, and not gaining or losing flesh: the excretions are effected in four ways—*per halitum*, *per cutem*, *per vesicam*, *per anum*, and are more highly oxidated than the food and drink consumed. The difference of oxidation of the ingesta and excreta, converted into work, should account for, and be equal to, the total labour effected per diem. This work is divided by the Professor into four heads: (*a*) the effort necessary to live, or the work spent on the performance of the organic functions [*opus vitale*]; (*b*) the work, converted into heat, requisite to keep up the animal temperature [*opus calorificum*]; (*c*) the mechanical work effected by bodily labour [*opus mechanicum*]; (*d*) the unknown, and hitherto unmeasured work done by the mind [*opus mentale*]. Confining himself to the subjects on which he has experimented directly, viz., the excretions *per vesicam*, and the mechanical labour of body, and work of mind, of which man is capable, he takes for granted that the substances excreted by the urine of a healthy man result from the wear and tear of tissue of every kind in the body; that, in a state of health, this excretion is exactly compensated for by the assimilation of an equal amount of the same substances ingested. Aware that, in restricting this hypothesis to the urine, he differed from many physiologists, he made a number of observations on the food taken by the persons experimented on, particularly with reference to the relative quantities of nitrogen received in their food, and excreted by the urine. The close agreement between the amount of nitrogen excreted *per vesicam* and *per anum*, and the amount obtained in analyses of the food, has induced him to assume that the nitrogen used in the body is naturally excreted by the kidneys, and that the surplus taken in food, and not used or required by the body, is thrown out as a merely superfluous excretion *per anum*. The quantity of nitrogen in food, and in the corresponding urine and fæces, being determined by direct experiment, the quantity excreted by the skin and lungs is found simply by difference.

The first set of experiments were made with a view of determining the daily discharge of urea in healthy urine of man; and this course of investigations was rendered necessary by the difference between various authorities in their quantitative estimates relative to this subject. Having made preliminary trials as to the different methods in use among chemists for the determination of urea quantitatively, Leibig's volumetric process was adopted, as calculated to give the most accurate and consistent results, the test solution of nitrate of mercury being estimated by a known weight of pure urea previously.

The subjects of the observations were required to live according to their usual mode, without any deviation from their regular habits, for a period of from five to seven days, before the constituents of their urine were estimated. The urine was in all cases that of healthy men, and the quantity operated on was the excretion of twenty-four hours, considering this to be the natural period of man's cycle of labour, food, and rest. The subjects for experiment were divided into two classes—1. Well-fed, flesh-eating, wine-drinking men; 2. Well-fed, water-drinking vegetarians.

The following tables contain the results of the experiments :—

Table A.—Beef-eaters.

No.	Urea per day.	Urine per day.	Specific gravity.	Urea per fluid oz.	Solids per day.
	Grains.	Ounces.		Grains.	Grains.
1	465·09	34	1023·8	13·70	817·25
2	677·25	62	1019·0	10·11	1065·69
3	644·62	52	1018·5	12·40	998·55
4	554·00	50	1015·8	11·08	818·00
5	630·00	45	1028·2	14·00	1330·20
6	484·30	41	1024·8	11·81	1051·25
Mean.	575·87	47·3	1021·7	12·18	1013·49

Description of the subjects of experiment :—

No. 1. Age, thirty-seven ; weight, 126 lbs. ; occupation, walks five miles per day, studies five hours per day ; food, 8 oz. fresh meat, 8 oz. white bread, 10 oz. vegetables.

No. 2. Age, thirty-five ; weight, 126 lbs. ; occupation, partridge shooting, office work three hours, anxious about business matters ; food, mixed diet.

No. 3. Age, nineteen ; weight, 126 lbs. ; occupation, studies five hours, walking and athletic games, two hours ; won the prize for the long jump at the Trinity College foot-races ; food, mixed.

No. 4. Age, thirty-nine ; weight, 174 lbs. ; occupation, walks two miles, office work, six hours ; food, mixed.

No. 5. Age, forty ; weight, 189 lbs. ; occupation, walks four miles, studies four hours ; food, mixed.

No. 6. Age, forty ; weight, 145 lbs. ; occupation, walks five miles, studies two hours ; food, mixed.

Table B.—Vegetarians.

No.	Urea per day.	Urine per day.	Specific gravity.	Urea per fluid oz.	Solids per day.
	Grains.	Ounces.		Grains.	Grains.
1	367·50	70	1014·00	5·25	1012·90
2	578·81	81	1014·76	7·14	1236·87
3	315·00	45	1015·23	7·00	712·50
4	366·12	56	1012·41	6·54	717·08
5	342·55	43	1017·17	7·87	775·60
Mean.	393·99	59	1014·71	6·76	890·99

Description of the Subjects of Experiment.

No. 1. Age, sixty-three; weight, 173 lbs.; occupation, walks three miles per day, office work one hour; food, 1 egg, 6 oz. bread, vegetables *ad lib.*, 16 fl. oz. milk, 40 fl. oz. water.

No. 2. William Wall, 16th Regiment (2nd battalion), in Military Prison, Dublin. Age, twenty-two; weight, 132 lbs.; occupation, three hours shot drill, one and a quarter hour ordinary drill, three and a half hours oakum picking; food—breakfast, 8 oz. oatmeal, ½ pint milk—dinner, 9 oz. Indian meal, ½ pint milk—supper, 8 oz. bread, ½ pint milk.

No. 3. William Tysack, Military Train, in Military Prison, Dublin. Age, thirty-one; weight, 146 lbs.; occupation and food, same as last.

No. 4. Thomas M'Donald, 18th Regiment (2nd battalion), in Military Prison, Dublin. Age, twenty-two; weight, 146 lbs.; occupation and food, same as last.

No. 5. Same man as No. 3, a fortnight subsequent.—*Med. Times and Gazette, Feb.* 7, 1863, *p.* 132.

109.—DIALYSIS AS APPLIED TO THE DETECTION OF POISONS IN MEDICO-LEGAL CASES.

By Dr. Edward R. Harvey, A.M., Lecturer on Physiological Chemistry at St. George's Hospital.

The following experiments were made in the laboratory of St. George's Hospital, to determine the value of dialysis as a method of separating poisons from organic mixtures, and thus rendering their detection a matter of ease to the medical practitioner. The universal interest that Professor Graham's remarkable investigations excited, and the hope that his discovery would prove available in medico-legal cases, made it desirable to ascertain by experiment if such hopes were securely based. Throughout the experiments Professor Graham's instructions were accurately observed. A dialyser formed of a gutta-percha hoop, twelve inches in diameter, upon which well-soaked parchment-paper was stretched (the whole forming an instrument like a tambourine), was floated in a glass basin of water. This water will be spoken of as the diffusate. To test the materials, a few preliminary experiments with coloured fluids were made, with satisfactory results. When the fluid on the dialyser was coloured by a crystalloid, the diffusate received the colouring matter; when a colloid was the colouring agent, the diffusate remained untinged. The decimal weights and measures were used throughout, as most convenient. The gramme is equivalent to 15·432 grains; the litre is equivalent to rather more than 1 pint 15 ounces; the centilitre, to 2 drachms 43 grains.

To avoid repetition, it may be stated at once, that whenever an organic mixture was placed on the dialyser, a little amorphous matter was invariably found, on evaporation, in the diffusate, at the end of twenty-four hours. How far this interfered with the application of the ordinary tests the experiments show.

Experiment 1.—A portion of the stomach of a man known to have been poisoned by opium was cut up, and, with its contents, mixed with water, and placed on the dialyser for twenty-four hours: the outer fluid, or diffusate, was then replaced by fresh water. At the end of the next twenty-four hours each diffusate was tested in the ordinary manner for meconic acid by sesquichloride of iron. No reaction occurred. The diffusates were then evaporated together to the bulk of an ounce: on this fluid being tested in the ordinary manner for meconic acid, a very faint reddish tinge was produced, but not of a sufficiently decided character to enable anyone to swear to the presence of opium.

Experiment 2.—0·500 gramme of powdered opium was digested in water, acidulated with hydrochloric acid, at 32° centigrade, for two hours, and then thrown on the dialyser. In twenty-four hours, the diffusate, without being concentrated, gave ample evidence of meconic acid.

Experiment 3.—One centilitre of laudanum was mixed with three eggs and water, a few drops of hydrochloric acid were added, and the whole thrown on the dialyser. The diffusate was evaporated to a bulk of about two ounces. Evidence of meconic acid was obtained, the reaction being as marked as that produced by fifty drops of laudanum in two ounces of water.

Experiment 4.—A centilitre of laudanum was mixed with three eggs and water, and thrown on the dialyser; no hydrochloric acid had been added. The diffusate, at the end of thirty-six hours, neither before nor after concentration yielded any evidence of meconic acid or morphia.

Experiment 5.—Two centilitres of laudanum were mixed with milk; a little hydrochloric acid was added. In twenty-four hours the diffusate had an opalescent appearance, and was so faintly tinged by the sesquichloride of iron solution that no evidence of meconic acid was obtained. The fluid was concentrated, and became turbid from phosphates. A drop of acid in great measure cleared the fluid. A very faint reaction of meconic acid was obtained.

Experiment 6.—Two centilitres of laudanum were poured down a rabbit's throat. The rabbit died almost instantly. The stomach with its contents were at once removed, and with acid and water thrown on the dialyser. No evidence of meconic acid could be obtained in the diffusate. It should be mentioned that the stomach had no smell of opium whatever when removed from the animal's body.

Experiment 7.—0·05 gramme of sulphate of strychnine was dissolved in water with a little sulphuric acid, and placed upon the dialyser for twenty-four hours. The faintest purple tint was momentarily produced with the bichromate of potash and sulphuric acid on a drop of the diffusate. A portion of the diffusate was concentrated, which gave more and more marked evidences of strichnine being present as it evaporated. Into the remaining portion of the diffusate, after the free acid had been neutralized, a full-grown frog was placed; at the end of three hours it was seized with tetanic rigors and emprosthotonos—quite characteristic of strichnine-poisoning upon these animals.

Experiment 8.—Six nux-vomica beans were cracked, and for thirty-six hours digested in very dilute sulphuric acid at 30° centigrade. The beans were now quite soft and had partially fallen to pieces, and the acid liquor had assumed a brown tint. All was thrown on the dialyser. At the end of twenty-four hours the diffusate was faintly brown in colour. No purple tint could be perceived until the fluid was concentrated, and the colour then produced was of a very uncertain nature: it was purplish running into pink. Experiments proved that organic fluids containing no strychnine gave an extraordinarily similar reaction with the same reagents. Further evaporation rendered the characteristic colour less distinct, until the fluid itself was a brown syrup totally unfit for any colour test. A few drops were placed under the skin of a frog, which after one or two slight convulsions became apparently comatose, and in twenty minutes died rigid. Another that was treated in the same way, except that the acidity of the syrup was neutralized, exhibited the same signs of torpor for forty-eight hours, when it died. The remaining syrup was diluted with water, neutralized by carbonate of soda, and a frog placed in it. In two hours emprosthotonos and tetanic convulsions came on. The fluid remaining on the dialyser was poured into a beaker and neutralized, and a frog placed in it, which was tetanized like the others in about two hours. One frog died in twelve hours; another was killed; and the third, after remaining on its back in a tetanic condition ten days, recovered. The soft broken beans were rolled up with fat into a bolus and given to a dog, upon whom they had no effect; proving that all the strychnine had been extracted.

Experiment 9.—0·1 gramme of strychnine, dissolved in hydrochloric acid and water, was mixed with three eggs, and thrown on the dialyser. So much organic matter besides strychnine was contained in the diffusate, that neither before nor after concentration could any evidence of strychnine be obtained. The diffusate was evaporated to dryness over water, and the strychnine extracted by ether. The extract after the ether had been driven off gave distinctly the reaction of strychnine.

Experiment 10.—0·070 gramme of sulphate of strychnine was given to a dog. In a quarter of an hour he had tetanic convulsions, and he died in half an hour. The stomach and contents, with acid and water, were thrown on the dialyser. The diffusate on evaporation yielded a purple tint, exactly of the same dubious character as in Experiment 8. It was evaporated over water to dryness ; a brown residue resulted : and a small piece of about the size of a pea was placed under the skin of a frog, which in two minutes was seized with violent convulsions, and died.

It did not appear necessary to carry the investigation further with organic poisons. All experiments led to the conclusion that small as was the amount of amorphous matter that passed into the diffusate, it was sufficient always to obscure, and sometimes, to prevent the ordinary chemical reactions, although, as was proved by the frogs in the strychnine experiments, the poisonous crystalloid was present in considerable quantity.

Experiment 11.—A small but unknown quantity of potassio-tartrate of antimony was mixed with three eggs and water, and thrown on the dialyser. In twenty-four hours the diffusate, on sulphuretted hydrogen gas being passed into it, became immediately of an orange tint, and soon deposited a yellow precipitate of sulphide of antimony.

Experiment 12.—0·200 gramme of arsenious acid was dissolved in hydrochloric acid, mixed with milk, and thrown on the dialyser for twenty-four hours; the diffusate was then replaced by fresh water, which remained in contact with the membrane forty-eight hours. Each diffusate gave proof of the presence of arsenic, but the second in a fainter degree than the first. All the arsenic was thrown down as tersulphide of arsenic by hydrosulphuric acid. To remove all organic matter, the precipitate was treated with ammonia, and, after filtration, re-precipitated by hydrochloric acid. The tersulphide when washed and dried amounted to 0·205 gramme, equivalent to 0·165 of arsenious acid.

Experiment 13.—0·010 gramme of arsenious acid was dissolved in hydrochloric acid mixed with milk, and placed on the dialyser for forty-eight hours. The diffusate was treated exactly as that in the preceding experiment. The tersulphide of arsenic obtained weighed 0·009, equivalent to 0·007 of arsenious acid.

Experiment 14.—A little arsenious acid (the quantity not weighed), with a few drops of water, was smeared on a cabbage leaf and given to a young rabbit. After a portion of the leaf had been eaten, the animal became uneasy, and was found the next day dead. The stomach, cut up, and contents, were digested in water at 30° centigrade, acidified with hydrochloric acid for twenty hours, and then thrown on the dialyser. Abundant evidence of the presence of arsenic was obtained in the diffusate without evaporation.

Experiment 15.—A rabbit was given on a cabbage leaf 0·020 gramme of arsenious acid. The stomach and contents, after the animal's death, was treated as before. From the diffusate was obtained 0·010 of tersulphide, equivalent to 0·008 of arsenious acid.

Experiment 16.—Two young rabbits, just purchased, were placed in a hutch which had been occupied some months before by rabbits poisoned by arsenic. Fifteen hours afterwards they were found dead, much of their food being untouched. Their stomachs and contents were treated as usual. The diffusate became of a tawny yellow on addition of sulphuretted hydrogen; and when concentrated a dirty-yellow precipitate resulted. This was separated, treated with ammonia, and filtered; the filtrate, which was yellow, on addition of hydrochloric acid was clouded with a light-yellow flocculent precipitate, soluble in ammonia, so far answering to the reactions of tersulphide of arsenic; but the amount was too small to make it worth while to attempt to obtain a metallic crust by reduction.

Experiment 17.—One gramme of chloride of mercury was digested for four hours with three eggs, hydrochloric acid, and water. The mixture was then put on the dialyser for twenty-four hours. The diffusate was replaced by fresh water, which remained for forty hours. On being concentrated, each diffusate gave strong evidence of mercury, the second containing less than the first.

Experiment 18.—Two dogs were given in meat chloride of mercury (the quantity unknown). They soon vomited up the meat, and continued to vomit mucus and other matter at intervals for some hours. Everything vomited was collected and digested, for forty-eight hours, with hydrochloric acid and water, at 30° cent. The meat had in great measure been dissolved, and the rest was in shreds and pulp, having evidently undergone artificial digestion. The whole was thrown on the dialyser. The diffusate gave strong evidence of mercury, both with the galvanic and chemical tests.

Experiment 19.—To discover if organic matter in any degree interfered with the dialysis of inorganic salts, 0·100 gramme of arsenious, dissolved in hydrochloric acid, was, with water, thrown on the dialyser, and treated as in Experiments 12 and 13. 0·120 gramme of tersulphide, equivalent to 0·96·5 of arsenious acid, was obtained; or 96·5 per cent. of the quantity dialysed. In Experiments 13 and 14, 82·5 and 70 per cent. were respectively obtained.

From these experiments, it appears that metallic poisons are detected far more readily by dialysis than are organic substances; that the presence of colloidal matter does hinder, to some extent, the separation even of the inorganic crystalloids; and that if colloidal matter be placed on a dialyser, a small quantity of it will be found, in twenty-four hours, in the diffusate. Thus, although dialysis may in some medico-legal cases be of assistance to the chemist, it would be dangerous to regard it as a substitute for the old and approved methods of analysis when poison is suspected.—*Lancet, Jan.* 3, 1863, *p.* 6.

110.—THE ALKALINE PERMANGANATES AS ANTIDOTES FOR ORGANIC POISONS.

By a writer signing himself MED. CHIR., in the British Medical Journal.

Notices of accidents by, and antidotes to, organic poisons, are constantly occurring in the medical journals. Your last impression contained three such articles—one on poisoning by aconite, and two suggesting antidotes to strychnine. Now, it would appear to me that we have not far to go in order to find substances which are calculated in an eminent degree to meet every case of poisoning by organic poisons. The alkaline permanganates are in all respects such substances. Since the introduction of Condy's fluid, which consists of combinations of these salts in solution, there no longer exists any difficulty in procuring them. This preparation is now to be found in almost every chemist's shop, and in very many households; and its virtues as an antidote, not only to offensive and infectious matters, but also to every form of organic poison, has only to be recognised in order to render it of ready avail when accidents occur from the swallowing of laudanum, strychnine, cyanide of potassium, aconite, or other such deadly substances. If this were more generally known, many valuable lives would doubtless be saved. The action of the permanganates on poisons of organic nature will be made clear by the following equations:—

Cyanide of Potassium and Permanganate of Potash. $KC^2N+^3(Mn^2O^7+KO)=6MnO^2+4KO+NO^4+2CO^2$.

Veratrim and Permanganate of Soda. $C^{34}H^{22}O6N+27(Mn^2O^7+CaO)=54MnO^2+^{27}CaO+^{22}HO+NO^5+^{34}CO^2$.

Strychnine and Permanganate of Magnesia. $C^{30} H^{16} O^{3} N + 26 (Mn^{2} O^{7} + M^{3}O) = 52 Mn O^{2} + 26 Mg O + 16 HO + NO^{5} + 30 CO^{2}$. —*British Medical Journal, Dec.* 20, 1862, *p.* 656.

111.—CASE OF POISONING BY VERATRUM VIRIDE.

By Dr. G. N. Edwards, Assistant Physician to St. Bartholomew's Hospital.

On Thursday evening, December 18, I was summoned to see a gentleman, a scientific chemist, who had taken, experimentally, one drachm of tincture of green hellebore (equal to about twelve grains of the powder). On my arrival, I found him sitting in the water-closet vomiting into the pan. His features were sunken; skin cold, and covered with a profuse, clammy sweat; his pulse quite imperceptible. He complained of intense pain about the epigastrium. The vomited matter appeared to consist at first of the food and contents of the stomach, afterwards of glairy mucus. I gave him immediately about an ounce and a-half of pure brandy, which at once checked the vomiting. At this time I was joined by Mr. Buxton Shillitoe, the patient's usual medical attendant. As he had slightly rallied, we had him moved into a room a few yards from the water-closet, and laid on the couch in front of the fire. The surface of the body was still very cold, especially the extremities. The clammy sweat continued, but the pulse could be felt indistinctly, beating, very feebly and irregularly, forty-four in the minute. A dose, containing sp. æther. suph. co. ʒss., and sp. ammon. arom. ʒj., was given, but immediately rejected by the stomach; but some brandy, given directly afterwards, was retained. A large mustard poultice was applied to the epigastrium, and a hot-water bottle to the feet. Two or three efforts at retching afterwards took place, and he once or twice vomited food, mucus, and a small quantity of blood. Warmth gradually returned to the surface, and the pulse became somewhat better in volume and power. Brandy was given at short intervals, and after about an hour a second dose of æther and ammonia, which was retained. The patient then fell asleep, slept for about a quarter of an hour, and awoke with the expression, "I am all right now," and appeared comparatively well. There was no diarrhœa throughout. He had two or three times a slight return of the symptoms; did not sleep during the night, but was quite easy; and the next morning only felt the discomfort arising from the mustard poultice and the soreness of the muscles caused by the retching. He then gave us the following account of his feelings during the attack:—

"Before taking the dose of the tincture, I had consulted

Pereira, and finding Dr. Mead's dose of the tinct. helleb. nig. to be two teaspoonfuls twice a-day, and knowing the green hellebore to be of the same tribe, I considered, in taking one teaspoonful, I was rather under than overdoing it. The drachm of the tincture was taken about half-past four or a quarter to five o'clock p.m., and my stomach soon afterwards whispered that I had admitted a troublesome guest. I went upstairs and sat down, thinking a cup of tea would set all right; but the uneasy constriction of the stomach continuing, with a tendency to sickness, made me retire to the water-closet. The sickness, once commencing, soon became alarmingly violent, with the most excruciating pain in the lower part of the stomach, the pain extending to about the size of my hand; the feeling of the seat of the pain was, that all the warm tea, water, &c., that I took to provoke the vomiting, went under the pain, making the constriction more and more violent. Finding the case getting desperate, I sent off for medical aid; the pain continued to increase, and the ejection from the stomach was now glairy mucus with blood, with running from the nose and eyes. The most painful and profuse cold sweating now came on, and the difficulty of breathing became more and more laboured. I could not help wondering at the presence of anything like heat or constriction of the throat; my mind was perfectly calm; and, although I thought it more than probable that I should die, I did not feel alarmed. Hearing and recognising the voice of one of my medical friends is all I recollected for some time afterwards. Immense circles of green colour appeared round the candle, which, as vertigo came on, and I closed my eyes, turned to red. The pain continued excruciatingly at the pit of the stomach, and a slight tendency to cramp existed when my legs were touched. It was some hour or more afterwards when I awoke, and found myself comparatively well. The sting of the mustard I can well recollect, and the horror of being forced to swallow brandy is even now with me. My bowels were not at all acted on. The next morning I took a dose of citrate of magnesia, and, in the evening, a blue pill, with colocynth, which relieved my bowels this morning."

Veratrum viride does not appear to have been much used in this country; but, in America, a pamphlet has been published on its employment, by Dr. Ephraim Cutter, of Woburn, Mass.; and several articles have appeared in the Medical journals respecting it. It is also described in the last edition of "Pereira's Materia Medica," where the symptoms from an overdose are stated to be precisely those observed in our patient. No authenticated case of poisoning by it has been recorded. The full dose of the tincture is eight minims.—*Med. Times and Gazette, Jan.* 3, 1863, *p.* 5.

112.—ON THE EXTERNAL EXHIBITION OF ATROPIA AND STRYCHNIA.

By Dr. Alexander Fleming, Physician to the Queen's Hospital, Birmingham.

I have for many years employed solutions of atropia and of strychnia for internal use, and prefer them to the galenical preparations of belladonna and nux vomica, because of their greater safety and efficiency. The solutions of both alkaloids which I employ are so proportioned in strength, that ten minims (by measure) is the ordinary commencing dose,—which admits of increase for the adult, and of lowering for the child.

The solution of atropia which I use is made thus :—Atropia, 1 grain ; distilled water, 5 drachms. Dissolve *thoroughly* with the aid of a few drops of diluted muriatic acid, and add of rectified spirit sufficient to make 10 drachms. This solution keeps well, and is of uniform strength. The tincture and extract of belladonna, however carefully prepared, vary much in power. I have found the tincture of one chemist seven times the strength of the same preparation from another and equally respectable chemist ; and the extract is even more uncertain. The internal, and at same time efficient, use of these preparations is for this reason very unsafe.

The solution is so proportioned that 10 minims, containing 1-60th of a grain of atropia, is the commencing dose for the adult. It should be given in a little water, once daily, at bedtime, and on an empty stomach. The dose is increased daily by 2 or 4 minims until a slight degree of the early physiological effects—dry throat, wide pupil, and dim sight—is produced. This is attained with much precision and safety ; but it may be necessary to increase the dose to 30, 40, and 50 minims, according to the strength of the patient. For children of one year and all ages under one year, the commencing dose is 1 minim, of two years, 2 minims, of three years, 3 minims, and so on up to ten years, when 10 minims may be given. In verifying the commencing doses for children I have been assisted by my late pupil Mr. Burnie, House-Surgeon of the Children's Hospital of this place.

The commencing doses here indicated for children and adults, are all fixed below what may be given with propriety in the majority of cases. While weak persons are readily influenced by atropia, much larger doses than those indicated are necessary in strong subjects, in whom, therefore, a few days are usually lost in the commencement of treatment, before the requisite dose is attained. In the diseases in which I use atropia, as epilepsy, asthma, constipation, and hooping-cough, this delay has no practical inconvenience. It is perhaps not superfluous to note here, that by minims I mean minims by measure. Ten minims of this solution of atropia are equal to eighteen drops, in which form it ought not to be prescribed. I generally order this solution alone, and direct the patient to be supplied with an Alsop's minimmeter to measure the dose.

I give it *once* daily. The action of one dose does not subside completely for sixteen or eighteen hours, and if a second be given before the effects of the first have passed away, we risk the production of cumulative action. When so exhibited the degree of action is less under control, and we may induce unexpectedly an alarming amount of atropism. The cumulative exhibition of atropia is not required to secure any of its therapeutical indications. It must be given *on an empty stomach.* The dose of atropia requires for its due action to be promptly absorbed; mixed with the contents of a full stomach it enters the system very gradually, and manifests its usual effects very imperfectly or not at all. This is one reason why the drug, when taken into the stomach of the rabbit, has no action. It meets there always a large quantity of food, and, mixing with it, enters the system very gradually. The kidneys, meanwhile, are busy eliminating it with the urine, and the atropia is never present in the body in sufficient force at any one time to cause its physiological action. Several experiments have satisfied me that this explanation applies to some at least of the other examples of the immunity of grassfeeding brutes to certain poisons. Their stomachs are always full. Lastly, I give the solution *simply diluted with a little water*, that it may pass quickly and easily into the blood. Atropia should never be given in pill, which may undergo solution very slowly or not at all, when two or three pills accumulating in the stomach or bowels, may, from some change in the gastro-intestinal fluids, be suddenly dissolved and excite severe atropism.

Atropia is sometimes employed internally in the form of valerianate, but this is not a convenient preparation. The valerianate is a very deliquescent salt, and forms a gummy mass, which it is difficult to weigh with accuracy. Moreover, it is a delusion to suppose that the valerianic acid represents in any sense the valuable medicinal properties of the valerian.

The solution of strychnia which I use is made thus :—Strychnia, 2 grains; distilled water, 5 drachms. Dissolve the strychnia *thoroughly* with help of a little diluted muriatic acid, and add of rectified spirit sufficient to make ten drachms.

This solution has the same advantages over the powder, extract and tincture of nux vomica, that the solution of atropia has over the tincture and extract of belladonna. It is uniform in strength, passes readily into circulation, and the dose can be apportioned with accuracy. The commencing dose is 10 minims, and contains 1-30th of a grain of strychnia. When employed for its *tetanic* action the solution should be taken in the morning, half an hour before breakfast, and in half an ounce of water, and the dose increased 2 or 4 minims daily until a slight degree of the physiological action—stiffness about the jaws or neck or spasmodic movements in the paralyzed muscles—of the drug is manifested, when no further increase should be made. As a tetanic it should be given only once daily, to avoid the risk of cumulative action by giving a second dose before the operation of the first has entirely subsided. It is taken in the morning, so that its action may be over before bedtime, and the sleep is not disturbed. Lastly, it should be given on an empty stomach, and diluted with water to ensure its prompt and easy absorption. Strychnia ought never to be given in pill. It is hard of solution in the weak acids of the stomach, and several pills may remain unchanged and accumulated there or in the bowels. A change in the secretions may then dissolve and transport them all simultaneously into the blood, and give rise unexpectedly to alarming tetanic symptoms. This is commonly the correct explanation of the so-called cumulative action of strychnia. It is the sudden solution and absorption of hard pills which have accumulated in the stomach or bowels.

When the strychnia is employed as a *tonic*, the dose of the solution is 5 minims, and it may then be exhibited twice daily with safety and advantage.

It would be a great convenience if all our solutions—and under that name might be included the stronger tinctures, as those of digitalis, aconite, opium, and Indian hemp—were made so as to have a commencing dose of 10 minims, which admits, as I have said, of easy lowering for the child, and when increased, does not involve the giving of too much spirit. By helping the memory, such an arrangement would promote materially efficient treatment. There can be few practitioners who are not conscious that in their daily practice, good opportunities of using active drugs are allowed to slip, because, at the moment of prescribing, the memory is not faithful as to the exact dose. A pharmacopœia which made the doses equal of all preparations bearing the same name would help to remove this difficulty.—*Edinburgh Medical Journal, Jan.* 1863, *p.* 625.

113.—ANTIDOTE FOR STRYCHNINE.

By M. Kurzak.

The experiments instituted by the author upon rabbits and dogs, place beyond a doubt the good effects of tannin as an antidote for strychnine. M. Kurzak concludes from his experiments that, if administered in time, tannin is an excellent antidote for this subtle poison. It is necessary to give twenty or twenty-five times as much of the antidote as of the poison; and in actual cases of poisoning it would be prudent to give much more. Infusion of galls may also be given with advantage. An infusion of black tea even is efficacious if the dose of strychnine taken was small; coffee has the same property, but in a less degree. Oak bark, which contains 8·5 per cent. of tannin, may also be given in decoction; it is convenient on account of the ease with which it may procured. The author also recommends as applicable to the same purpose from their richness in tannin, acorns, chestnut bark, willow bark, tormentilla root, bistort root, and the root of the carnation.—*Journal de Chimie Médicale* and *Pharmaceutical Journal.—Dublin Medical Press, Nov.* 12, 1862, *p.* 479.

114.—EFFECTS OF POISONS ON MUSCLES.

Strychnine, as is well known, excites powerful tetanic spasms in muscles. Coniene causes paralysis, which, in man, has been shown to commence in the lower extremities and proceed upwards. Other poisons influence more especially particular muscles: such as aconite, which operates on the heart; lead, on the extensors of the arms; cantharides, on the urinary bladder; secale cornutum, on the pregnant uterus, &c. Bernard showed that with curara the motor nerves might be paralysed without affecting the muscular contractility; and Kölliker has demonstrated that veratrine destroys the contractility without affecting the motor nerves. Both poisons operate through the circulation.—*Lancet, Feb.* 7, 1863, *p.* 141.

115.—CASES OF OPIUM POISONING TREATED BY BELLADONNA, WITH REMARKS.

By Dr. William F. Morris, Resident Physician to the Pennsylvania Hospital.

Case 1.— ——, æt. nineteen, druggist, was admitted into the Pennsylvania Hospital Feb. 21st, 1862. On the morning of the 21st he came to the shop of his employer as usual, where he remained till after 10 o'clock, when he went to Rosengarten and Son's chemical works, and there purchased an ounce of the sulphate of morphia. He then walked into the outskirts of the city where he obtained some water, poured it into the bottle containing the morphia, and after having swallowed a large mouthful, recorked the bottle and put it into his pocket. This bottle was subsequently obtained by his employer, who carefully evaporated the contents to dryness, and found seventy-five grains missing from the ounce. The patient insisted that none of the contents of the bottle had been spilled. He afterwards wandered about the town for an hour and a half, when he began to feel tired and sleepy, and being near a friend's house, went in, stating that he had poisoned himself, and that he wished an interview with his sister before he died. He now appears to have become frightened at the step he had taken, and in a few minutes after, although he was very sleepy, and his gait staggering, walked to a neighbouring drug store, and obtained and swallowed two drachms of tannic acid, with a view of counteracting the effects of the morphia. His friends being much frightened, at once sent for his employer, who soon arrived and administered a quantity of sulphate of zinc. Dr. W. F. Atlee, a short time after, repeated the dose, which then caused free emesis. He also gave a strong decoction of coffee with two grain doses of Squire's extract of belladonna, in solution, which were repeated at short intervals, until in about an hour he had taken twenty grains of the extract. When admitted into the hospital at 2.15 p.m. his pupils were contracted to a pin's point, his pulse 80 and soft, his gait staggering; slow of speech, although if sharply questioned, he would answer his name. He complained of thirst, and would sleep even when standing up if those supporting him did not keep him constantly in motion. He was at once sent into the garden, where two attendants kept him walking briskly. Ten grains of Herring's extract of belladonna in solution were then administered, and this was repeated in half an hour (3 o'clock). The pulse had now risen to 100, but the pupils were still excessively contracted, his intellect was, however, more active, and he now gave a tolerably clear account of himself, complaining of thirst. At 3.30 the pupils were beginning to dilate, though he continued still very drowsy. At 3.45 his condition remaining unchanged, the pupils continuing about the same size, and no flush of the face being perceptible, ten grains more of the extract of belladonna in solution were given, making in all fifty grains, thirty of which were given after his entrance into the hospital. At 4.5 there was a marked change for the worse; he could no longer support his own weight, and the attendants who had charge of him dragged him along; his pulse was 120, and his pupils widely diluted. He was now carried up stairs, stripped and put under a cold shower bath, which revived him so much that he struggled strongly to get away; he was rubbed briskly with coarse towels till dry and the skin red. The good effects of the bath were but transient, for even before he was thoroughly dried, drowsiness again overtook him, and vigorous shakings with repeated attempts to make him walk entirely failed. He was therefore placed in bed. His respiration was laboured and very slow. At this period Dr. F. G. Smith, the attending physician, saw him, and directed the application of the galvanic battery, one pole being placed over the cervical vertebræ, and the other over the diaphragm. It was then attempted to cause him to swallow some brandy with two grains of sulphate of quinia in solution, but this failed, a portion running into the larynx and exciting cough. Half an ounce of brandy was then given by injection, and at the same time mustard plasters were applied to the abdomen and inside of the thighs. Under the action of the battery the frequency of the respiration was much increased.

At 5.30 the pupils were fully dilated, respiration 11 per minute, pulse 112, capillary circulation more active, and a slight red flush of the face showed the full action of the belladonna. The injection of brandy was repeated at 6.30 and 7.30, but there was no material change in the symptoms. At 8.30 the pulse was found to be more feeble, the lips bluish, and respiration more laboured. The temperature of the surface was not much reduced. The galvanic battery was now reapplied, and under its influence the number of respirations rose to 13 per minute; this was followed by an injection of brandy, and sulphate of quinia. At 9.45 the injection was repeated; he then opened his eyes and looked around, but would not answer when spoken to, and soon relapsed into his comatose condition. His pupils continued dilated, and did not contract under the influence of light. Respirations were 12 to the minute and not quite so stertorous. Pulse 114 and stronger, the skin was warm and the flush had disappeared from the face. The mustard plasters having acted well had been transferred to the back and chest, where having thoroughly reddened the skin, they were now removed. At 11.30 the skin was warm, lips red, pupils still dilated and immovable, breathing stertorous, 10 per minute. The battery was again reapplied and the number of respirations increased to 12, pulse 108. He roused up and made some resistance during the application of the battery, mumbling "What are you about?" but at once on cessation of the current dropped off into stertorous slumber. At 1 o'clock a.m. he had so far recovered that the nurse was unable to give him his injection on account of his violent resistance; he refused to take anything unless he could see the doctor's prescription for it. At 2 he was able to talk, and promised to swallow the medicine which was directed for him. The nurse reports his having vomited a dark-coloured liquid during the last hour, his pupils are still dilated and immovable, pulse 114, respiration 12 per minute. Half an ounce of brandy was now given him by the mouth, and a wine-glass of milk punch was ordered for him at 3 and 5. His respiration continued stertorous. At 8 o'clock he was awake and rational, his pupils somewhat dilated, pulse 114, full and strong, respirations 16 to the minute, easy and natural. At 9.30 his condition was much the same, his bowels, however, had been moved, the evacuation being normal in character. He has also had much bilious vomiting, to relieve which he took lime-water and milk. At dinner time he took some beef essence, which he retained, and has had no vomiting since. His skin was still considerably reddened from the application of the mustard. From this period he continued steadily to improve, and on the next day had regained his usual health and appetite.

Case 2.—J. K., æt. 55, was admitted into the Pennsylvania Hospital on the 24th of March, 1862. He had two small superficial wounds in the neck, and a larger one at the bend of the left elbow, exposing but not wounding, the median cephalic and basilic veins. He was brought into the house about 5 o'clock p.m., surly, and rather soporose, with a weak pulse. It was stated that his wounds were the result of an attempt at suicide at his place of business early in the morning, and that not succeeding in this, he had at about 9 o'clock swallowed the contents of an ounce phial of laudanum. An emetic was at once administered to him, consisting of thirty grains of sulphate of zinc with an ounce of the wine of ipecacuanha, followed by a small quantity of warm-water. In about three-quarters of an hour this produced slight vomiting. At 6 o'clock five grains of the extract of belladonna in solution were administered. This was repeated at 7 o'clock, at which period no change was remarked in his condition, except that he had become more sleepy, and could be kept awake only by having an attendant constantly to shake and talk to him. An hour later (8 o'clock) he had become still more sleepy, and the dose of belladonna (five grains) was repeated, but it was with great difficulty that he could be made to swallow it; he would put it to his lips, and before he had swallowed more than a few drops, would again fall asleep. At half-past 8, his attendants were unable to keep him awake. At 9, his pulse was very feeble and rapid, his breathing stertorous, his pupils—as had been the case from the time of his admission —contracted, but not to a pin's point. Two grains and a half more of the extract of belladonna were now administered, large mustard plasters were applied to the front of the chest, and he was subjected to a strong current of electricity from an electro-magnetic battery, one pole being placed over the diaphragm, the other on the cervical vertebræ. His pupils had now become dilated, showing the full action of the belladonna. Notwithstanding these remedies, his respiration grew slower; at 10, the pulse at the wrist could not be felt (auscultation over the heart, however, showed it to be 120). A little later even under the stimulus of the battery, his respirations were only seven per minute. Artificial respiration was now employed, causing expiration by pressure on the anterior part of the thorax, and allowing inspiration to take place through the natural resiliency of the parts. Small doses of whiskey, frequently repeated, were administered. It was with great difficulty, however, that he could be made to swallow it. Under this treatment he slowly improved; at 2 o'clock a.m. his pulse was 120, and could easily be felt at the wrist; respiration still stertorous, sixteen to the minute. At 4, 8, and 9 o'clock no change was noted, except that his pupils were less dilated than they had been the night before. At the latter hour, milk-punch every half-hour, with 20♏ spts. ammoniæ aromat. was ordered. His condition remained much the same up to 12 o'clock, when he suddenly sank, and a few minutes after expired. No post-mortem examination could be obtained.

The first of the preceding cases presents several points of interest. 1st. The length of time which intervened between the taking of so large a quantity of morphia, and the appearance of the symptoms, which mark the second or soporose stage of the action of that drug. 2nd. The recovery of the patient after taking so large a quantity of the poison. 3rd. The action of the belladonna.

1. *The length of time which intervened between the taking of the large dose, and the appearance of the symptoms.*—In regard to the first point, Mr. Taylor, in his work on *Poisons*, remarks: "When any one of the salts of morphia is taken at once in an overdose, the symptoms are strongly marked, and they follow each other more speedily. They generally commence in from five to twenty minutes after the poison has been swallowed." Although this be the rule, yet the volume of the same author furnishes a remarkable exception to it, for at page 547, he relates the case of M. Bonjean, who swallowed fifty-five grains of the acetate of morphia, dissolved in an ounce of water, in whom "no symptoms of any importance manifested themselves until an hour after the poison was taken, and then there was simply giddiness, with a tendency to sleep. Two hours after the occurrence, he had still the power to answer questions! In four hours deep coma supervened.

Christison quotes a case from Orfila where twenty-two grains of the muriate of morphia were taken, in which four hours elapsed before the patient first felt approaching stupor. Half an hour later he could still answer questions, although he could not see the interrogator. This delayed action is seen in other preparations of opium, and the same author relates the case of an habitual drunkard, who swallowed two fluid-ounces of laudanum, "while intoxicated to excitement from beer and spirits," where no material stupor appeared for five hours. He also adduces a case taken from Corvisart's Journal, where two and a half fluid-ounces of laudanum, with one drachm of extract of opium (*i.e.*, 213½ grs. of opium) were taken, without producing well-marked stupor till after the lapse of more than an hour.

2. *The recovery of the patient after so large a dose.*—The largest recorded dose of any of the salts of morphia from which any patient has recovered, that I have been able to find, is the case of M. Bonjean (above quoted) who took fifty-five grains of the acetate of morphia in solution. Taylor also gives a case where fifty grains of the same salt were taken on an empty stomach, and another where twenty grains were swallowed, both of which recovered. *Christison* (pp. 558–559) mentions an example of recovery from twenty-two grains of the muriate of the same alkaloid.

3. *The action of the belladonna.*—Belladonna, under the different names of στρυχνος μανικος, solanum furiosum and belladonna, has been described by both ancient, mediæval, and modern writers. As it is an ornamental plant, growing in temperate climates, and having berries which possess a sweetish taste, there have been many cases of poisoning by it reported, principally from the accidental eating of the berries; and the symptoms of poisoning from its use seem to have been almost as familiar to the old authors as to us of the present day. A knowledge of the antagonistic powers of opium and belladonna seems also to be of early date. Pena and De Lobel, in their work entitled Stirpium Adversaria Nova, so early as 1570, give an account of Italian peddlers who excited the wonder of the common people by giving, to alleviate thirst, portions of the root of the belladonna, the evil effects of which were averted either by vinegar, wine, or theriaca. In 1661, Horstius, in his Opera Medica, relates the case of a man who swallowed a spoonful (*cochleare plenum*) of the inspissated juice of the belladonna, by mistake for rhob. sambuci. This was followed by dimness of vision, dryness of the throat, delirium, and tremors, from which he is stated to have recovered, after taking the theriaca, with the juice of rue. In the work of Faber on Strychnomania, published in 1677, thirteen cases in which belladonna berries were swallowed, are recorded; of these, two escaped without any unpleasant symptoms, but the remaining eleven were affected with delirium, dimness of vision, difficulty of swallowing, and a number with redness of the skin. Out of this number, there were two deaths; in both the delirium passed away, and coma preceded death. The treatment in ten of these cases consisted chiefly of theriaca, with adjuvants, which varied more or less in each case. He also quotes a case from Brotbe-

quius, a contemporary, of a similar kind, in which recovery took place after the exhibition of opium. In 1766, M. Boucher, of Lille, published five cases of poisoning by belladonna berries; his treatment consisted chiefly in the administration of emetics, purgatives, enemata, and vinegar; the latter he looked upon as an antidote to belladonna. In two cases, however, one of which was in a state of coma, and the other delirious, preparations of opium were administered. I have not been able to find any further reference to the subject until 1810, when Joseph Lipp published an inaugural dissertation, entitled, "*De veneficio baccis belladonnæ producto atque opii in eo usu.*" This paper I have been unable to consult, but Giacomini, in his *Traité de Matière Médicale et de Thérapeutique*, states that in it are recorded several cures by means of Sydenham's laudanum. The latter author, in his article on opium, remarks that it has constantly been found useful in the treatment of poisoning by hyoscyamus, stramonium, and belladonna, and asserts "*Les Italiens ont donné dans ces cas l' opium a haute dose et ils ont vu la stupeur, le délire et les convulsions disparaitre.*" In a paper read before the Physiological Society of Edinburgh, by Dr. Thomas Anderson, in 1854, he remarks that, following out the suggestion of Dr. Graves, that an agent which would dilate the pupils might be administered with advantage in cases of coma with contracted pupils, in low fevers, it occurred to him to use belladonna in opium poisoning, and he there details two cases successfully treated by it.

[Dr. Norris then gives a tabular list of the cases of opium poisoning treated by belladonna, which have been reported in the Journals during the last few years.]

M. COURTY, treating of the use of the same alkaloid in hypodermic injections, remarks: "Nevertheless, in the small number of cases where the atropic intoxication has given origin to cerebral symptoms which have appeared to require treatment, opium in doses of twenty-five milligrammes (*i. e.* 0·28 grain) every half hour hastened the return of the functions to their normal state, by neutralizing in a manner the effects of belladonna upon the brain. One or two pills has ordinarily sufficed for a cure." (*Ann. de Thérapeutique*, par A. Bouchardat, 1860, p. 40).

In addition to the cases referred to in the foregoing table, where opium and belladonna have been given with a belief in their antidotal powers, several interesting observations are recorded where these agents in large quantity have been administered simultaneously through accident. Christison relates the case of a lady poisoned by three successive injections into the vagina, each containing "the active matter of a scruple of opium and half an ounce of belladonna leaves." Three hours afterwards she was insensible and motionless, the face pale, pupils dilated, pulse frequent and small, and the breathing hurried. He remarks: "Here the opium seems to have prevented the delirium usually induced by belladonna in the early stage, while on the other hand the belladonna prevented the usual effect of opium upon the pupils, and actually produced the opposite action." The patient entirely recovered. Cazin gives an instance where six grammes of Sydenham's laudanum with two grammes of tinct. belladonnæ (equivalent to vini opii f℥j, ♏xlv; tr. belladonnæ f℥j, U. S. P.) with forty grammes of the oil of sweet almonds, intended as a liniment, were by mistake swallowed. This large dose caused only somnolence, injection of the face and conjunctiva, and dilatation of the pupils. Dr. Coale reports a case where a child aged nine years swallowed two suppositories containing four grains of extract of belladonna and four grains of opium with very slight effect. The reporter, however, "supposes it possible" in this case that as dinner had been eaten a short time before, this may have retarded absorption. Newman records a case where minute portions of a belladonna plaster accidently swallowed immediately dispelled the soporific effects of a previous dose of morphia and caused some of the symptoms of belladonna poisoning.

The foregoing cases seem conclusively to show that in opium poisoning, belladonna in doses which in a state of health would certainly poison, may be administered with impunity and be followed by a rapid subsidence of the symptoms produced by the exhibition of the former drug, and *vice versa*, that opium rapidly and safely counteracts the poisonous influence of belladonna. The treatment above indicated has, indeed, in some cases failed, and this may prove that they are not mutually specifics: but even in these fatal cases (which are few) we may sometimes see a partial amelioration of the symptoms, and it is well worthy of inquiry how much in these instances the relative quantities of the two drugs administered, the stage of poisoning in which the patient was first seen, the age and constitution, may have contributed to the result.

Finally, how do they counteract each other? The mode of action of these drugs on the nervous system is not at present well understood, although several able essays have been written upon it. To those interested in the toxicology of these prominent narcotics I would especially refer as well worthy of perusal the papers of Mr. Hughes *On the Significance of the Contraction and Dilatation of the Pupil produced by Opium and Belladonna respectively*; Mr Harley *On the Physiological action of Atropine in Dilating the Pupil*; of Fuller *On the Administration of Belladonna, and on certain Causes which modify its action;* and of Lee *On Opium and the Mydriatics.—American Journal of Medical Science, October,* 1862, *p.* 395.

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RETROSPECT OF MEDICINE,

1860—1862.

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D. I. ROEBUCK, PRINTER, 14, TRINITY STREET, LEEDS.